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SCIENTIFIC SUBJECTS.

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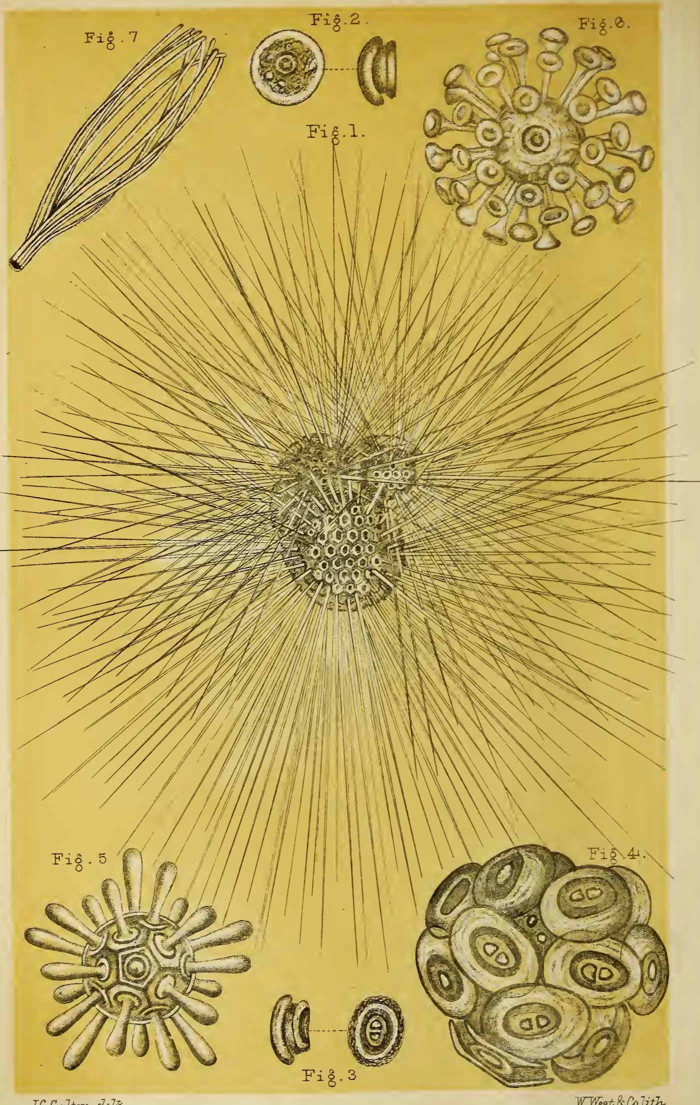


J. C. Galton delt. W. West & Co. lith.

From the CHALLENGER'S Dredge.

POPULAR SCIENCE REVIEW.

IN THE WAKE OF THE "CHALLENGER."

By JOHN C. GALTON, M.A. (Oxon), F.L.S.

PLATE CXXIX.

"Sie est die einzige Künstlerin: aus dem simpelsten Stoffe zu den grössten Contrasten; ohne Schein der Anstrengung zu der grössten Vollendung; zur genauesten Bestimmtheit immer mit etwas Weichen überzogen, ... ihre Werkstätte ist unzugänglich."

GOETHE, *Die Natur*, 1780.

"Geheimnissvoll am lichten Tag,
Lässt sich Natur des Schleiers nicht berauben,
Und was sie deinem Geist nicht offenbaren mag,
Das zwingst du ihr nicht ab mit Hebeln und mit Schrauben."

GOETHE, *Faust*, 1er *Theil*, 1801.

"Was man an der Natur geheimnissvolles pries,
Das wagen wir verständig zu probiren,
Und was sie sonst organisiren liess,
Das lassen wir krystallisiren."

GOETHE, *Faust*, 2ter, *Theil.*

DOCTOR FAUSTUS, seated among his dusty tomes, his crucibles, retorts, and limbecks, speaks with becoming reverence of the secrets of Nature, which, it appears, he has only attempted to induce her to yield up by employment of the art of the alchymist; while his former pupil, the priggish Wagner, flushed may be with his success in creating the "Homunculus," treats of these same secrets with a somewhat flippant tongue. Nowadays, in these matter-of-fact times, we, but with no diminished regard for Nature, gently, but firmly and with confidence, exact of her tribute from those of her treasures which she has buried fathoms deep, beneath the waves of the ocean, not strictly perhaps "with levers and with screws," but with dredge, trawl, and sounding-lead.

The first successful attempts to ascertain the nature of the sea-bottom and the limit of life at depths greater than 100 fathoms were made by Sir J. Ross, in 1818, who, by the help of a machine of his own invention, brought up several pounds of mud from 1,050 fathoms, or above $1\frac{1}{4}$ mile, in Baffins Bay, 72° 30′ N., 77° 15′ W. This deposit was described by Ross and Sabine, who accompanied him. It was a fine greenish mud, but no accurate determination of its nature was made. Ehrenberg, in 1853, examined the surface scum and mud obtained by Penny in 73° and 74° N., and found it to consist of—(*a*) Diatoms (vegetable) living at the surface; (*b*) Radiolaria (animals), also surface-living; and (*c*) sponge-spicules (animal) from the bottom. In 1854, Bailey determined the nature of the mud procured by Brooke in 900–2,700 fathoms in the Sea of Kamschatka. The mud was purely silicious, there being absolutely no calcareous organisms. These and other observations tend to establish the existence of a circumpolar zone of silicious deposits in the Arctic regions, within the parallel of 55° N., viz. a *North Polar silicious cap.* In 1839 an Antarctic expedition was despatched under Sir J. Ross, attention having been awakened by the appearance of Ehrenberg's work on minute organisms ("Die Infusionsthierchen"), from the years 1836–38. This naturalist discovered that organisms whose skeletons resembled those occurring in the cretaceous and tertiary rocks, and sometimes constitute their whole mass, are still living. The observations of Dr. Hooker and of Sir J. Ross at two distant points of the Antarctic zone proved the existence of a *South Polar silicious cap.* The discovery of an *intermediate zone* of *calcareo-silicious* deposit, in about 110° lat., of a deep sea sediment, dates back to 1853, and is due to Ehrenberg's examination of soundings brought up by Berryman from 2,000 fathoms, between Newfoundland and the Azores. These consisted of silicious diatoms, *Radiolaria*, and sponge-spicules, as in the Antarctic seas, but the bulk was calcareous *Foraminifera.* From this Ehrenberg concluded that "chalk is nothing but a mass of dead foraminiferal skeletons." These results have been confirmed by Bailey, Huxley, and others, as well as the fact of the extension of a similar deposit over the South Atlantic and into the Indian Ocean. The geological nature of the greensand deposit formations was determined by Ehrenberg in 1854; and the discovery of newer greensand deposit, in 100–300 fathoms was made almost contemporaneously by Pourtales. Parker and Jones, too, ascertained that a formation of greensand was going on in the Australian seas. It is in extending some of the conclusions already established, and in the discovery of much that is wholly new to science, that the *Challenger* has been for the last three years employed.*

* These preliminary remarks regarding the discoveries of Ehrenberg and others are taken from a lecture by Prof. Huxley, "On the Recent Work of

The objects of the expedition are perhaps best expressed by the following abstract from the "Scientific Orders" of the *Challenger*:—"The principal object of the proposed expedition is understood to be to investigate the physical and biological condition of the great ocean basin."* The route proposed was as follows:—Down the coast of Portugal and Spain; across the Atlantic, from Madeira to the West India Islands; thence to Bermuda, then to the Azores, Cape de Verde Islands; along the coast of South America, across the Atlantic to the Cape of Good Hope. Thence by Marion Island, the Crozets, and Kerguelen's Island, to Australia and New Zealand, going southward *en route*, opposite the centre of the Indian Ocean, as near as may be, with convenience and safety, to the southern ice-barrier. From New Zealand, through the Coral Sea and Torres Straits westward, between Bali and Lombok; thence through Celebes and South Seas to Manila. Then eastward into the Pacific, visiting New Guinea, New Britain, and the Solomon Islands. Afterwards to Japan, where some time might be profitably spent. Thence the course should be directed across the Pacific to Vancouver's Island, southerly through the eastern trough of the Pacific, and then homewards round Cape Horn. Special attention must be paid to the fauna and flora of Marion Island, Crozets, and Kerguelen's Islands, and of any new groups to be met with in the region to the south-east of the Cape of Good Hope. If possible, the Aucklands, Campbell, and Macquarie Islands should be touched at. The zoology of the sea between New Zealand, Sydney, New Caledonia, and the Fiji and Friendly Islands should be carefully investigated, as it is possible that the Antarctic fauna may be found here at accessible depths.

Physical Observations.—These are to be made at stations the position of which have been carefully determined, and chosen, so far as possible, at equal distances apart. At each station should be noted the time of the observation, the weather, the temperature of the surface of the sea, the depth, and the bottom temperature, by means of two Miller-Casella thermometers,† with the specific gravity of both the surface and of the bottom water. The nature of the bottom is to be determined by an apparatus to bring up samples, and, if possible, by the dredge. When practicable the amount and nature of the gases contained in the water should be determined. In the path of

the *Challenger* Expedition and its bearing on Geological Problems," delivered at the Royal Institution, on Friday evening, Jan. 29, 1875.—See "Proc. Royal Inst.," vol. vii. part 5, April 1875.

* "Nature," vol. vii. pp. 191 and 252.

† This instrument, and its copper case, are figured at pp. 291, 292 of Prof. Wyville Thomson's "Depths of the Sea," Lond. 1873.

currents *serial* temperature should be taken, either with the instrument of Siemens'* or with a Miller-Casella thermometer. Simple determination of the depth of the sea at regular distances is of primary importance.

The surface temperature of the sea as well as of the air (determined by dry and wet bulb thermometers) is to be recorded every two hours. The records obtained should be reduced to curves.

In the North Atlantic there seem to be the following strata:—

(*a*) *Superficial*, of which the temperature rises with the atmosphere. This does not exceed 100 fathoms in depth.

(*b*) An *upper* stratum beneath this; the temperature of which slowly diminishes as the depth increases to several hundred fathoms. In the higher latitudes this is considerably *above* the normal of latitude, but in intertropical regions it is considerably below the normal.

(*c*) A stratum in which the rate of diminution of temperature is rapid, after being 10° in every 200 fathoms.

(*d*) The whole of the deeper part of the North Atlantic below 1,000 fathoms is believed to be occupied by water not many degrees above 32° Fahr. The *glacial* stratum.

An interesting question is the extent to which colder, and therefore heavier, water may run *up-hill* on the sides of declivities. The position of the Azores is favourable for the determination of this.

The greatest results are expected from the Southern Ocean. The specific gravities of surface and of bottom water are to be compared, and that of intermediate depths, when serial soundings are taken.

The degree of transparency of the water should be determined by Siemens' photographic apparatus, and by lowering a white plate to measured depths.

Observations should be made on the relation, discovered by Professor Schouw, between barometric height and the latitude of the observation. It seems that there are certain meridians of high and of low pressure.

The thermometer and barometer should be observed every two hours. Tidal observations may be made by means of a graduated pole.

It is desirable, when enough tidal observations have been made, to settle the mean level of the sea, and that a permanent beach-mark should be established, recording the date and height above such mean level.

Samples of sea-water should be collected at various depths.

* Mr. Siemens' instrument is very useful for serial measurement, as it does not require to be hauled up for each reading.

A portion is then to be boiled *in vacuo*, the gases collected, and their volume determined, part being hermetically sealed in glass tubes. The gases contained in the air-bladder of certain fishes should be examined.

Botany.—The duties of the botanist are twofold: 1. To collect. 2. To make observations on the life, history, and structure of certain plants. The vegetation of oceanic islands is of especial importance, these being sometimes the last position of flora of great age, which are liable to be speedily exterminated, as at St. Helena. A collection should be made from each islet of a group, as the floras of contiguous oceanic islands are wonderfully different. Among islands of which the flora is absolutely or almost unknown are:—Fernando de Noronha; Trinidad and Martin Vaz, off Brazil; Socotra; Prince Edward I.; Crozets; Marion I.; Pitcairn, Bounty and Macquarie Islands. With regard to the Indian Archipelago, Java alone has been explored, and the Philippines but partially. Collections from islands east of Java, especially Lombok and New Guinea, would be very valuable. The part played by icebergs in the transportation of plants is of importance, and the algæ in hot-springs should be examined.

Zoology.—The littoral fauna of the Papuan shores of Torres Straits is important for comparison with the fauna of the opposite Australian shore.

A hydrographical examination should be made of "Wallace's line" in the Malay Archipelago, and of the littoral faunas on either side of it. Soundings should be taken in the Celebes Sea, Capt. Chimmo having found in 2,800 fathoms a mud bottom of *Radiolaria* and sponge-spicules, but no calcareous organisms.

A line of soundings between Japan and Vancouver's Island, and between the latter and Valparaiso, would be useful for investigation of the quadrant-like zone of the Pacific which separates the eastern boundaries of the Polynesian archipelago from the coast of Northern Asia and America.

The limitation of reef corals should be investigated. It has been suggested that the limit of twenty fathoms may be a thermal one. Specimens of the hair of unmixed races of men should be collected.

Before entering generally and in detail into the results of the expedition, so far as they have been accomplished, it will be interesting to follow, as best we can, in the wake of the gallant ship during her cruise, picking up such "flotsam and jetsam" as may be accorded to us.

At 11.30 A.M. of Dec. 21, 1872, H.M.S. *Challenger* cast off the jetty at Portsmouth. There was a strong breeze blowing from the S.W., and the storm-drum was hoisted. This ship, liberally lent by Government for the purposes of science, is a spar-decked * corvette of 2,000 tons' displacement, her tonnage being greater than that of all the other ships together which formed the expedition of Cook in 1772. Sixteen out of the eighteen 68-pounders which formed her armament had been removed, so that the main deck was almost clear for scientific action. Besides private cabins there is a commodious zoological workroom (see Fig. 1), a chemical and physical laboratory, and a dark room and working room for the photographers. The weight of the ship is so great that there can be no "give and take" between it and the dredge, tending thus to jerk the latter off the ground. This will probably be met by using a rope of a length greatly in excess of the depth to be explored, and by attaching weights to it. Dredging and sounding are to be carried on from the mainyard, and not, as in the cruise of H.M.S. *Porcupine*, from a derrick at the bow or stern.† A strong pennant is attached by tackle to the end of the yard, a compound arrangement of fifty-five Hodges' "accumulators" ‡ being hung to the pennant, and beneath it is a block through which the dredge-rope is run. This arrangement is better than dredging from a derrick.

The navigation of the ship was under the charge of Captain Nares, R.N., Professor Wyville Thomson being at the head of the scientific staff, which consisted of Mr. H. N. Moseley, M.A. (Oxon), as botanist; the late (unfortunately) Dr. Rudolf von Willemoes-Suhm, a distinguished pupil of Professor von Siebold, of Munich, as zoologist; Mr. Buchanan as chemist; Mr. Murray as geologist; and Mr. Wild as artist and private secretary to the director of the scientific staff.

With regard to the furniture of the zoological laboratory, §

* This is an advantage, according to Professor Wyville Thomson.—"Nature," vol. vii. p. 386.

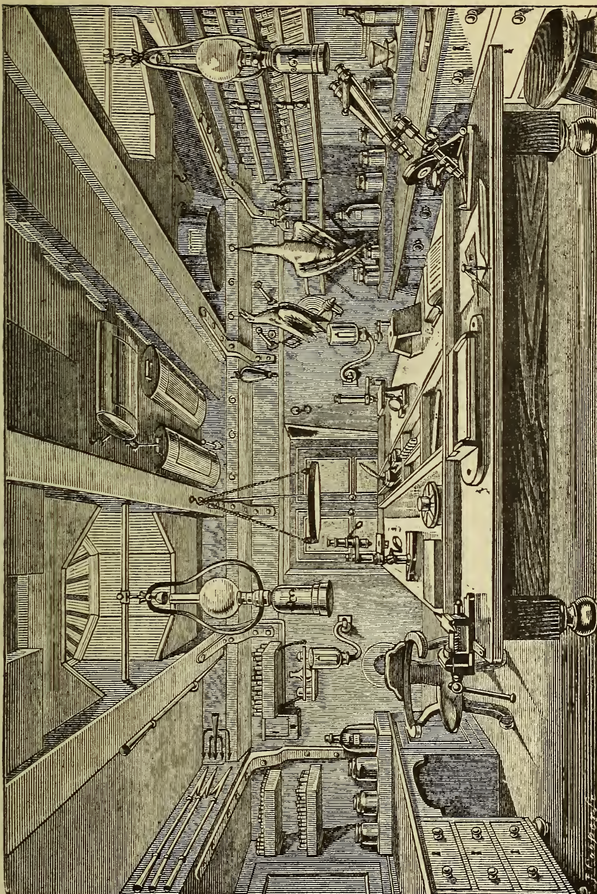
† This arrangement is figured in Professor Wyville Thomson's "Depths of the Sea," fig. 46, p. 248.

‡ This is figured in an article by W. Lant Carpenter, "On the Apparatus employed in Deep Sea Explorations on board H.M.S. *Porcupine*, in the Summer of 1869."—"Popular Science Review," vol. ix. p. 286. This machine itself is well described in some humorous verses sent to the "Cape Monthly," by one "Jack Skylight":—

"It ain't a bad doge neither; for when it's pulled it stretches,
And gives a kind of surge when the dredge at summut ketches:
It's like a concertina, Bill, but where the wind is squoze,
From end to end a set of stays like Inde-rubber goes;
A block is tacked at bottom, and through it runs the line," &c.

§ See the first of a series of letters sent by Dr. von Willemoes-Suhm to Professor Siebold, which appeared in "Siebold und Kölliker's Zeitschrift," Bänd. xxiii. to xxv. inclusive.

Fig. 1.



the supply of microscopes was most liberal, comprising three of Hartnack's pattern, four of Merz's, one of Winkel's (of Göttingen), a binocular by Smith and Beck, an instrument by Ross, one dissecting microscope by Zeiss, besides simple microscopes for dissecting, lenses, &c. Such instruments as forceps and scissors were plated with nickel, so as to guard against rust—that plague of all steel instruments, either on board ship or in hot climates. These are much to be recommended to all naturalists who work on the sea-coast.*

Of the library the naturalists were not a little proud, including as it does *inter alia* such splendid works as Bronn's "Klassen und Ordnungen des Thierreichs," Milne-Edwards' "Leçons sur la physiologie et l'anatomie comparée," ten volumes of Petermann's "Mittheilungen," Siebold and Kölliker's "Zeitschrift," Max Schultze's "Archiv," the "Transactions" of the Linnæan and Zoological Societies, and some volumes of the "Philosophical Transactions," together with such reprints of papers upon the development of marine animals as were the property of individual members of the staff.

The naturalists spent their time generally in the laboratory, until they were hailed by the cry, "The dredge is up," when they rushed on deck, each armed with a forceps, eager to explore the contents of the dredge or trawl. In some respects the trawl was found to be superior to the dredge, as this latter brings up so much mud as to spoil delicate organisms.

For a week the *Challenger* experienced very bad weather in the Channel and in the Bay of Biscay, which, however, was even an advantage, as tending to find out weak points before that it was too late to remedy them. The ship rolls very much —over 35° "when put to it."

Off the coast of Portugal specimens of *Hyalonema* † and *Euplectella* ("Venus' Flower Basket") were obtained with spicules not to be distinguished from those of their representatives in the Philippines. Some fishes were brought up from 600 fathoms off Cape St. Vincent, which were in a peculiar condition from the expansion of the air in their bodies, on relief from extreme pressure, their eyes having a singular appearance, "protruding like globes from their heads." From 1,090 fathoms a gigantic Amphipod crustacean was brought up, of the family *Hyperina*, allied to *Phronima*. The eyes were very remarkable, "extending in two great facetted lobes over the whole of the anterior part of the cephalo-thorax," like the eyes

* They were made by Baker, of Holborn.

† This is beautifully figured in Professor Wyville Thomson's work already quoted, fig. 66, p. 421.

of one form of trilobite.* Some beautiful little urchins were also obtained, allied to the *Salteria* taken by Count Pourtales off Florida. "To an advocate of the 'continuity of the chalk' it is pleasant," says Professor Wyville Thomson, "to see in the flesh this little beauty, which has hitherto been reckoned among the lost tribes."

On Friday evening, Feb. 14, 1873, Santa Cruz was left, the weather being fine, and a light breeze blowing from N.E. Course westward. On the morning of the 17th, from a depth of 2,700 fathoms a mutilated specimen of a new Gephyrean was obtained, which was referred by Dr. Suhm to a new genus, *Leioderma*, intermediate between the Sipunculids and Priapulids. On the 18th, with 2,220 fathoms of line out, there was brought up, attached to a kind of coral, a sponge belonging to the *Hexactinellidæ*, but the type of a new genus *Poliopogon* (πολιός, white; πώγων, beard) *amadou*. Both surfaces were covered with a network of square meshes like that of *Hyalonema*. From its base projected a bush of anchoring spicules, each having a barbed end like the anchors on the skin-spicules of it.

Professor Wyville Thomson remarks † that in hot and calm weather the towing-net is usually unsuccessful, for it seems that the majority of pelagic forms retire during the heat of the day to the depth of a few fathoms, and come up in the cool of the evening and in the morning, and, in some cases, in the night. The larger phosphorescent animals are often abundant during the night around and in the wake of the ship, while none are taken in the net in the day. On the afternoon of the 26th no less than 3,600 fathoms of rope were payed out, the deepest haul by several hundred feet which had hitherto been made. For a few previous soundings part of the mud had been getting darker, having, too, less calcareous matter on analysis, and fewer foraminifera were seen under the microscope. Now the latter organisms were entirely wanting, there being only a clay mud, like chocolate, remaining for days suspended in water. When at last it settled it formed a smooth red-brown paste, with no feeling of grittiness to the fingers, "as though it had been levigated with extreme care for a process in some refined art." It was almost pure clay on analysis, consisting of a silicate of alumina, sesquioxide of iron, with a

* See Dr. Suhm's paper "On some Atlantic Crustacea from the *Challenger* Expedition." "Trans. Linn. Soc.," New Series. "Zoology," vol. i. Pl. XI. fig. 4.

† "Nature," vol. viii. p. 29. A family of silicious sponges with six-rayed spicules.

small quantity of manganese. It would be interesting to know whether this is connected with the extreme depth. Professor Wyville Thomson thinks not. The depth at Station 5 was 2,700 fathoms, and on that occasion foraminifera were abundant, and several bivalve molluscs were captured living. The difference in depth cannot be so effective as to arrest the life of organisms to the secretion of whose testes the grey Atlantic ooze is due. He is inclined to attribute this deposit to a movement of the water from some special locality—possibly the mouths of the great South American rivers.

On Sunday, March 2, the first patches of Gulf-weed were seen. On the morning of the 4th a fine decapod crustacean was caught, having the characters of *Astacidæ*, but differing in the absence of eyes and eye-stalks.* The absence of eyes, remarks Professor Wyville Thomson, in many deep-sea animals, and their full development in others, is very remarkable.†

A singular absence of higher forms of life has been noticed for some days past. "Not a bird was to be seen, from morning to night." More latterly sharks and dolphins have been seen, the latter in pursuit of flying-fish. They are easily deceived by a rude imitation of one of their prey, such as a white spinning bait, when the ship is going rapidly through the water. On the 11th the dredge-line was paid out to more than 4,000 fathoms,

* *Vide* Dr. Suhm's paper "On some Atlantic Crustacea from the *Challenger* Expedition," Pl. XIII., fig. 1. *Willemoesia leptodactyla* is its right name, the generic name *Deidamia*, under which it is figured in "Nature," vol. viii. p. 51, fig. 2, being already the property of a North American genus of *Sphingidæ*. "*The eyes are entirely wanting*, nor is there, as in *Astacus Zaleucus*, any place left open where you might expect them." . . . . "It is very astonishing indeed that, among all crustaceans known to us, *Willemoesia approaches most closely to the fossil Eryontidæ*."

† For instance, a stalk-eyed crustacean from 700 fathoms in which the eyes are well developed in shallow water—*e.g. Ethusa granulata*—may have eye-stalks gradually modified into immovable pointed organs, devoid of special sense, while *Munida* from a like depth has unusually developed and sensitive eyes. "It is possible that in certain cases as the sun's light diminishes the powers of vision become more acute, while at length the eye becomes susceptible of the stimulus of the faintest light of phosphorescence." Absence of eyes is not unknown among the *Astacidæ*. *A. pellucidus* from the Mammoth Cave, in Kentucky, is blind, and from the same cause, but morphologically, the eyes are not wanting, being represented by rudiments, while in *Willemœsia* there is no trace of them or their peduncles. Mr. Wood Mason describes ("Nature," June 5, 1873, p. 111) a macrovrous crustacean, a type of a new genus—*Nephrops Stewarti*—dredged from 250 to 300 fathoms off the east coast of the Andaman Islands, which had lost its organs of vision by disease, but in compensation the anterior and auditory organs were greatly developed. It burrows in mud at the bottom.

nearly five statute miles. A tube-building annelid, making tubes from the sparse gritty matter of the red mud, was brought up. It was allied to the genus *Owenia*, but had no cephalic branchia. "As bearing upon some of the most important of the broad questions which it is our great object to solve, I do not see that any capture which we could have made," says Professor Wyville Thomson, "could have been more conclusive than that of this annelid." The depth was practically 3,000 fathoms, a depth which does not appear to be greatly exceeded in any part of the ocean. The nature of the bottom was very unfavourable to higher animal life, and yet this creature, closely allied to a well-known shallow water group of high organisation —the *Clymenidæ*—was not developed.

In dredging off Sombrero Island, on March 15, several sponges belonging to the *Hexactinellidæ* were brought up, closely allied to those obtained off the coast of Portugal; showing that the distribution of this remarkable order is very wide. Two crustaceans, belonging to the family *Astacidæ*, were also captured, both totally devoid of eyes; the one was a *Willemœsia*,* the other *Astacus zaleucus*.† Both were carefully examined by Dr. Suhm. Where the eye would be seen in *A. fluviatilis* "there are two round vacant spaces which look as if the eyestalk and eyes had been carefully extirpated, and the space they occupied closed with a chitinous membrane."

On March 16 H.M.S. *Challenger* arrived at St. Thomas, after 30 days' voyage from Teneriffe, having completed 23 stations. The natural history of this island, which belongs to Denmark, is fairly known, large collections having been sent to Copenhagen. Ophiurideans are particularly plentiful here. While dredging for algæ Mr. Moseley brought up some specimens of a flowering plant, apparently *Halophila*; a genus hitherto known only to occur in the Red Sea and in the Indian and Pacific Oceans.‡

St. Thomas was left on the 24th, and the Bermudas reached on April 4, 32 stations having been completed. It may be worth mentioning that on March 26, in sounding at 3,875 fathoms, two Miller-Casella thermometers, which had been sent down, *sicut est mos*, with a ship water-bottle, attached to a "Hydra" sounding instrument, came up broken. The mischief was traced to the giving way of the smaller, unprotected bulb. § Why should the

* *W. crucifer*, T. L. S., *Ibid.* pl. xii. fig. 10; and "Nature," vol. viii. p. 266.

† T. L. S., *Ibid.* pl. x. fig. 1.

‡ This plant was submitted to Prof. Ascherson of Berlin, who said that it was a congener of *H. ovalis*, and called it *H. Baillonis*. It was found half a century ago by Bertero.—"Journ. Linn. Soc. Botany," vol. xiv. p. 311.

§ A figure of the fragments is given in "Nature," vol. viii. p. 110.

mischief occur here, and how is it to be remedied? It is probable that the end of the small bulb was the last point of the instrument heated and sealed after the tube had been filled with liquid, and that, consequently, the annealing at this point was imperfect. It will be of no use, thinks Professor Wyville Thomson, to protect this bulb in the same way as the large bulb, by an outer shell; the only plan, therefore, is to thicken the small bulb and improve its temper. It should be noted that the instrument had undergone a pressure of four tons to the square inch, though it had only been tested to bear three tons to the same area.*

With regard to the Bermudas, one would imagine at first sight that the islands exhibited on a small scale "an epitome of the geological phenomena of a disturbed palæozoic district." General (when Lieutenant) Nelson, R.E., has pointed out that the great proportion of the Bermudas is formed simply by the blowing up by the wind of a fine calcareous sand, a product of the disintegration of coral, shells, and other constituents of the neighbouring reefs. This sand is blown into dunes 50 feet high ("Æolian formation," Nelson)—see fig. at p. 267, "Nature," July 31, 1873—which move inland, "forming shoreward a glacis at the angle of repose of loose sand." This sand is converted into a rocky material by the agency of rain-water, which, containing a quantity of free carbonic acid, dissolves the lime freely. This solution of bicarbonate of calcium, on percolating, loses again a part of its carbonic acid, and deposits a cement of carbonate between the particles of coral sand. There is a total absence of running water in the Bermudas, nor is there a trace of a pool or even of a ditch; for the rain percolates, as through a sieve, the ground, which is also horizontally porous, thus letting in the salt water also, below the sea-level. The terrestrial vegetation of the Bermudas may, according to Mr. Moseley,† be divided into five principal stations, each having a flora peculiar to itself; viz. 1. To the coast-line, with the littoral flora. 2. Peat-bogs or marshes. 3. Shallow brackish ponds. 4. The caves. 5. The remaining land surface. Along the coast-line there occurs abundantly *Borrichia*, a composite, in two forms, side by side; one with succulent bright green leaves, the other with glaucous and downy foliage. The binding plant of the dunes is a hard prickly grass (*Cenchrus*). In

* It seems that Messrs. Negretti and Zambra had some years previously—in 1857—made upwards of fifty of the same modification of Six's thermometers for the Board of Trade. See their letter in "Nature," vol. viii. p. 529; also a communication, accompanied by a figure, "On a New Deep Sea Thermometer."—"Proc. Roy. Soc." vol. xxii. p. 238.

† "Journ. Linn. Soc. Botany," vol. xiv. No. 77, pp. 317–321.

the marshes the ferns are the great features, especially two *Osmundas*. Here were found nearly all the lichens and fungi. The peat burns well, and has the appearance of its ordinary European representative. In excavating Bermuda dock a bed of lignite was found at 50 feet depth, evidently an ancient peat-bog. In the caves the coffee grows wild, the tree being of large size. The juniper forms the main feature of the vegetation. Common fennel, too, has spread all over the islands. About 160 species of flowering plants were collected in the Bermudas, of which no more than 100 are indigenous. Those of West Indian origin were transported, as Grisebach suggests, by the Gulf-stream, or the general drift of surface-water. The occurrence of American plants is probably due to vast numbers of migratory birds which come from that continent. These, among them the American golden plover (*Charadrias marmoratus*), probably bring a number of seeds, either attached to their feet or feathers, or temporarily lodged in their digestive tract. A ship laden with grapes had been lately wrecked on the coast. Some of the seed germinated, so that General Lefroy was enabled to obtain a small number of vines for his gardens.

The *Challenger* left the Bermudas on April 21, and arrived at Halifax on May 9, having worked through 44 stations. After a stay of a few days, the ship returned to the Bermudas, 55 stations having been explored. On Thursday, June 12, the *Challenger* left the Bermudas *en route* for the Azores. On this voyage some fine specimens of a magnificent barnacle were hauled up, attached to curious nodules, consisting almost entirely of peroxide of manganese, much resembling certain nodules dredged up 700 miles eastward of Sombrero Island. This cirriped,* *Scalpellum regium* (fig. 2), is by far the largest of known living species of the genus, a female specimen having an extreme length of 60 millimetres, of which the "capitulum" was 40 more, and the "peduncle" 20 more in length. The latter was covered with imbricated scales and coarse hairs, and the valves were 14 in number. In two of the specimens there was no trace either of testes or of intermittent organ, but the ovaries were well developed. In nearly all "complemental males" were to be seen, from five to nine in number, attached within the "occludent margins," in a fold of the body-sac quite free from the valve. The male (fig. 3) is very simple, being oval and sac-like, and about 2 inches in length. It has no rudiments of valves, nor is there a trace of a jointed thorax to be seen, even after boiling in caustic potash. The

* See article "Barnacles: their Facts and their Fictions."—POPULAR SCIENCE REVIEW, Oct. 1873.

whole of the posterior two-thirds of the body is filled with sperm sacs.*

With regard to the "Sargasso Sea," the masses of "Gulf" weed which float here are not matted together, but consist of a single layer of the feathery branches of *Sargassum bacciferum*, the vesicles of which plant are encrusted with a beautifully netted white polyzoön. The patches of weed, varying in shade

Fig. 2.

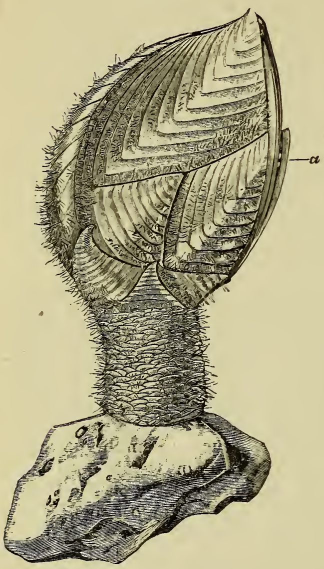
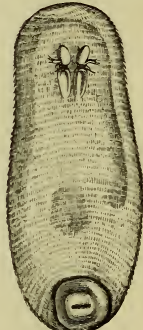


Fig. 3.



from an olive to "a golden tint of olive," form a lovely contrast with the "intense indigo" of the sea between. One of the most perfect examples of protective resemblance is shown by the fauna of the "Gulf" weed, which imitate both in form and colour their habitat, so as effectually to deceive both birds and fishes who might otherwise prey upon them. A little fish —the *Antennarius*—makes nests in the weed, and a small crab which swarms upon the "sargasso" corresponds in degree of colour with whatever part it may chance to inhabit.

* Apropos of this discovery, Mr. Charles Darwin makes, in a long and interesting letter in "Nature," Sept. 25, 1873, some remarks on the males and complemental males of certain cirripedes.

On Friday evening, July 4, the *Challenger* arrived at Porta Delgada, the capital of the island of San Miguel, one of the easternmost of the Azores group. At Furnas, in San Miguel, there are two sets of boiling springs which were explored by the botanist of the expedition for algæ. Connected with one spring there is a basin full of intensely hot water always in ebullition. This had no algæ in it. Some diatoms were found in a hot sulphureous spring, probably derived from a cool spring which mingled with it. Certain *Oscillatoriæ* were also discovered in water so hot as to scald the hand. Unfortunately no exact record of temperature could be made, as no thermometers were at hand which registered sufficiently high.* A peculiar substance of the consistence of Indiarubber was found floating on one of the hot-springs. It burnt like the substance which it counterfeited, and in doing so emitted a similar smell. It resembled, too, as far as Mr. Moseley could remember, a peculiar elastic substance, a specimen of which is in the Kew herbarium among the lichens found upon the shores of an Australian lake. By some it has been held to be a mineral, and on this assumption some specimens are under Professor Maskelyne's care, in the British Museum.

The *Challenger* left the Azores for the Cape de Verde Islands on Wednesday, July 9. The Island of St. Vincent, one of the principal of the group, was visited. It has a flat central tract surrounded by high land, the former part being evidently the bottom of an ancient crater. The abundant plant is *Lavandula rotundifolia*. The tomato (introduced) has run wild everywhere. At the time of the visit there had been no rain for a year, but the island is said to become green after rain as if by magic. The rocks about tide-mark are covered with a band of incrustation composed of calcareous algæ. This incrustation is of several colours, white, pink, or cream-colour.

At St. Paul's Rocks, as at St. Vincent, there is a pinkish incrustation about tide-mark upon the rocks.† A green alga, which is constantly being loosened by the surf from the bottom, and floats upon the surface, is gathered by the "noddies" (*Sterna stolida*) for building their nests, but is not used by the "boobies" (*Sula fusca*). A few bushels of guano are to be found in rock-hollows, which is almost wholly soluble in nitric acid; but no diatoms—which abound in Peruvian guano—are

* For some remarks by Mr. Archer on these algæ, &c., see "Journ. Linn. Soc. Botany," vol. xiv. p. 328.

† Darwin, in his "Naturalist's Voyage," mentions that there is a similar incrustation on the coast of Ascension, resembling certain cryptogamic plants (*Marchantiæ*) often seen on damp walls. Of this he gives a beautiful woodcut.

to be found in the residue. St. Paul's Rocks are detached miniature islands about 1° N. of the equator, and having a longitude of 29° 15′ W., and are 500 miles from the nearest point of South America, lying about midway between that and the African coast. The rock approaches serpentine in structure. Not a trace of land vegetation, not even a lichen, is to be found here. "It is a remarkable fact," says Darwin, "that all the many small islands, lying far from any continent, in the Pacific, Indian, and Atlantic oceans, with the exception of the Seychelles and this little point of rock, are, I believe, composed either of coral or of erupted matter."*

On Aug. 22, St. Vincent's Island was left by the *Challenger* for Bahia, to make her fourth section across the Atlantic. Bahia was reached on Sept. 15, after a successful but stormy voyage. Fernando de Noronha was stopped at, *en route* for Bahia, Sept. 1st and 2nd being spent here. The entire length of the whole chain of islands is about seven geographical miles, the group being situated in 3° 50′ S. lat., and being distant from Cape St. Roque, the nearest point in South America, more than 200 miles. On the main island there is a remarkable column of "phonolite," or clinkstone, more than 1,000 feet high, called "The Peak." The island is volcanic. The Cape gooseberry and the castor-oil plant grow wild here. The plant mentioned by Darwin as covered with fine pink flowers, but without a single leaf, is a euphorbiaceous plant—*Jatropha gossypifolia*, Linn. "Its bare stem," remarks Mr. Moseley, "and branches render it a striking object amongst the green of the creepers when the forest is viewed from the sea." The flora is not very rich in species, and the same plants occur everywhere. There are neither ferns nor mosses, nor liverworts, though moist and shady places are not wanting, and lichens are very scarce. Mr. Darwin observes that the volcanic island of Fernando de Noronha, placed in many respects under nearly similar conditions, is the only other country where he has seen a vegetation at all like that of the Galapagos islands.†

After a stay of a little more than a week at Bahia the *Challenger* left for the Cape of Good Hope, touching at Tristan d'Acunha on the way thither. This group of islands has an area of about sixteen square miles, although stated by Grisebach (*Vegetation der Erde*) to be only two square miles in extent. A plant resembling a chrysanthemum—*Chenopodium tomentosum*—with strong-scented leaves, is used by the inhabitants, a decoction of it, under the name of "tea," being drunk with milk and sugar. It grows abundantly on Inaccessible Island, which is distant about 23 miles from Tristan. Inaccessible Island is

* Darwin's "Naturalist's Voyage." † Ibid.

covered with "tussock" grass (*Spartina arundinacea?*) which forms an enormous penguin "rookery," being so thick as almost to be impenetrable. This grass has a habit, like that of the Falkland Islands (*Dactylis cæspitosa*), of growing to five or six feet, springing in tufts, and forming massive boles or clumps at its base, composed of contorted root-fibres matted together. These are so tough as to require an axe to cut them. The grass thrives best where it is saturated with penguin dung. The close similarity of the three islands of the Tristan group points to a former close connexion. The presence of many plants here may be accounted for by the existence of a Cape Horn current which comes sweeping up to the islands. Part, too, of the Brazilian current, which turns from the coast of South America, brings many seeds, which, however, being tropical, do not germinate. These are known to the settlers as "sea-beans," from the belief that they grow at the bottom of the neighbouring sea. One of these is the bean of a leguminous tree common at Bahia, while another is identical with a seed cast up upon the Bermudas, which, under the same name, is worn as a "curiosity" on watch-chains. The thrush and bunting, the only strictly land birds which inhabit the islands,* feeding, as they do, upon berries, have also assisted in spreading plants from one island of the group to another.

By the time that the month of November had come round the *Challenger* had arrived at Simon's Bay, Cape of Good Hope. After a stay of about six weeks here the journey was resumed southward, † Prince Edward's Island being the first halting-place, though no landing could unfortunately be effected here. After leaving the Cape dredgings were taken a little to the southward at depths of from 100 to 150 fathoms. The animal life was found to be abundant, the fauna being generally like that of the North Atlantic, many species even being identical with those on the coasts of Britain and Norway. From two dredgings between Prince Edward's Island and the Crozets at depths of 1,375 and 1,600 fathoms, it was further demonstrated that here, in the south of the Indian Ocean, we have principally to do with the same deep sea fauna as the Atlantic Ocean presents. From the station between the above two groups an Ostracod was obtained in comparison with which all previous ones are pigmies, its shell having a length of 25 millimetres and a height of 16 millimetres. It is said that one of a

* A coot, according to Darwin ("Naturalist's Voyage"), is also to be found here; from which he deduces that "the waders, after the innumerable web-footed species, are generally the first colonists of small isolated islands."

† I have not succeeded, despite of some pains to ascertain them, in finding out the exact dates of the arrival at and departure from the Cape.

smaller size, *Cytherina Baltica*, exists in the transitional mountain ranges of Gothland.* From this and other similar cases it is suggested that at great depths gigantic forms of genera and families have been preserved which never attain such a size at the surface and in shallow waters.†

At Marion Island a halt was made, and Mr. Moseley had a day at his disposal for collecting plants. The flora was found to be very similar to that of Kerguelen's Island, but was poorer in species.‡ No landing was made at the Crozets. On Jan. 7, 1874, the *Challenger* arrived at Kerguelen's Island, and remained in the neighbourhood till Feb. 1. Here only two plants were found new to the flora, viz. a *Cerastium* and an *Uncinia*. Large collections were made of the famed Kerguelen's Land cabbage (*Pringlea antiscorbutica*, Hooker), and of *Lyallia Kerguelensis*.§ From observations on Kerguelen's Island and elsewhere it is concluded that the higher forms of crustacea are almost entirely absent on the shores of the Antarctic islands, while in deep water here (as has been already witnessed at 300 fathoms) they are present in almost the same abundance as in the tropics. It is also concluded that the characteristic crustacea of the surface-water fauna of the Antarctic islands belong to the *Isopoda* and *Amphipoda*; that is, to forms which carry the ova in pouches up to their full period of development. As the Echinodermata, too, show, as has been observed by Professor Wyville Thomson, an unusual number of species in which the young develope direct in the maternal pouch, conditions must exist which

* Second letter from Dr. Suhm to Professor Siebold, dated Sydney, April 1874. "Siebold and Kölliker's Zeitschr." Bd. xxiv.

† For instance, the President of the Linnæan Society received from Professor Wyville Thomson a drawing of a specimen of a Gymnoblastic Hydroid "of such colossal dimensions that the largest form hitherto known sinks in comparison with it into utter insignificance." It was brought up by the trawl on June 17, 1875, in the North Pacific, from a depth of 2,875 fathoms. The "Hydranth" was 9 inches across from tip to tip of the expanded (non-retractile) tentacles, and the "Hydrocaulus" was 7 ft. 9 in. high! "That the enormous depths," remarks Mr. Allman, "from which this colossal Hydroid has been brought up should favour the development of gigantic representatives of the diminutive forms of shallower zones, and that in the tenants of these sunless regions of the sea we should find colours not less vivid than those of their light-loving relatives, are facts full of significance." See "Nature," vol. xii. p. 555.

‡ "Journ. Linn. Soc. Botany," vol. xlv. p. 387.

§ The fauna and flora of this island have also been described lately by the naturalist attached to the Venus Transit Expedition. See "First Report of the Naturalist attached to the Transit of Venus Expedition to Kerguelen's Island, Dec. 1874," by the Rev. A. E. Eaton. "Proc. Royal Soc." vol. xxiii. p. 351.

are unfavourable to a free-swimming stage of development.* Corinthian Bay, in Yong Island, of the Heard group, was reached on the evening of Feb. 6, but, unfortunately, a change of weather prevented a stay here of more than two hours. "Numerous glaciers come right down to the shore. The vegetation is most scanty, most of the land surface not covered with ice being bare." Only five flowering plants, apparently of the same species as at Kerguelen—among them a much-dwarfed *Pringlea* and one or two mosses, and a liverwort—were found.†

From deep soundings (1,375 to 1,900 fathoms) taken before reaching the Crozets the bottom was found to be composed entirely of *Orbulina* and *Globigerina* dead, and of the same species as those on the surface. Some Coccoliths and Rhabdoliths were also found. Samples of these sea-bottoms were of the purest carbonate of lime yet obtained; and when placed in a bottle and shaken up with water they looked like a quantity of sago. South of Heard Islands the bottom, as shown by soundings at 1,260 fathoms, was quite different, "being one mass of diatoms."

The most southerly station was reached on Feb. 14, being in lat. 65° 42′ S., and long. 79° 49′ E.—just outside of the Antarctic circle, the "threshold" of which is at 66° 32′ S. On Feb. 24, in trying to get under the lee of an iceberg, the *Challenger* was brought by a strong current into collision with it, and had her jib-boom carried away. At this time the ship was really in some danger from the numerous icebergs. On the following day the *Challenger* was within 15 miles of the position of the so-called "Termination Land" laid down on a chart sent by Lieut. Wilks to Capt. Ross. As no signs of it were to be seen it was concluded that its existence was mythical.‡ Laboratory work in the southern latitude was very unpleasant, the microscopes and other instruments being so cold that it was anything but agreeable to handle them, in rooms in which the temperature averaged 25° F. for several days. Dredging, too, was a critical operation, as the gear, becoming stiffened, was liable to part. Berg and floe ice were examined, and were found to contain the usual diatoms. In 48° 18′ S. lat. the fauna was already fully that of warmer zones. The Indo-Australian current therefore extends farther southwards than had hitherto been expected. Indeed, certain surface-animals of the warm Indian current from the N.W. were observed as soon as 50° 15′ lat. S. was reached.§

* Von Willemoes-Suhm.—"Proc. Royal Soc." vol. xxiii. p. 351.

† Moseley. Ibid.

‡ "The Antarctic. A letter from H.M.S. *Challenger*." By Prof. Wyville Thomson, in "Good Words," July 1875. See also a communication "On Dredgings and Deep-sea Soundings in the South Atlantic, in a Letter to Admiral Richards, C.B., F.R.S."—"Proc. Royal Soc." vol. xxii. p. 427.

§ Von Willemoes-Suhm. Ibid.

On March 17 the *Challenger* anchored near Melbourne. The next point reached after Australia was New Zealand. After leaving Port Nicholson, on July 7, the ship proceeded along the eastern coast of New Zealand, and on the evening of the 19th arrived at Tongatabu. On the 22nd the *Challenger* left, and on the 24th anchored off Matuku Island, in the Fiji group. Here a most valuable "take" was made in the shape of a living specimen of the pearly nautilus (*Nautilus pompilius*), which came up in the trawl from the depth of 300 fathoms, It was kept alive in a tub for some time, in order that its habits might be watched. According to Dr. Willemoes-Suhm it is very common in shallow water, and the natives capture it upon the reefs with baskets made up for the purpose. Like the turtle it is a dish, but so choice that the chiefs alone are allowed to indulge in it.* Among the remarkable pelagic animals captured was a naked petropod, in which the "ptera," or wings, were completely absent, their places being supplied by two large conical processes, each of which carried a large black eye at the tip. It is probably the *Pelagia alba*, incompletely described by Quoy and Gaimard. Near the Kermadek Isles the Pyrosomata were so abundant that the sea resembled "a dark carpet covered with large luminous balloons."

On July 28 the *Challenger* arrived at Levuka, the capital of the Fiji Islands, having touched at Kandavu, and on August 3 returned to the latter place, remaining there till the 10th. In this region the impression was confirmed that "while species differ in different localities, and different generic types are from time to time introduced, the general character of the fauna is everywhere very much the same."† Api, one of the least known of the New Hebrides group, was the next halting-place, the *Challenger* arriving at the edge of the reef on August 18. The inhabitants—who were almost entirely naked, of forbidding countenance, and armed with clubs, spears, and poisoned arrows—were mistrustful, and did not encourage intercourse. Large ships scarcely ever touch here, and next to nothing in literature of this island is known. Empty beer bottles, it is said, are the favourite articles of barter with the natives. Raine Island, which was reached on August 31, was the next place of halt. The ship had been accompanied by Mother Carey's chickens ever since leaving Api, but a search for their nests on Raine Island was unsuccessful. This island has been well described by Jukes in his "Voyage of H.M.S. *Fly*." A land crab (*Ocypoda*), like the *Grapsus* on

* Letter to Professor Siebold, dated Cape York, Sept. 1874. Ibid., Bd. xxv.

† Wyville Thomson. "Nature," vol. xi.

the rocks of St. Paul, is abundant in the sand-dunes which are the ramparts of the island, and preys upon the young and eggs of the sea-swallows which swarm here. The same dead turtle was, it is believed, actually seen which Jukes describes as being in a position as though attempting to scale the rocks. The breeding-place of the frigate-bird was found in the middle of the island; but it is not quite certain whether this bird is the *Fregatta aquila* (Finsch and Hartlaub) or *F. minor*. The tropic bird (*Phaëton phœnicurus*) here breeds in holes on the ground, while in the Bermudas its breeding-place is in holes in perpendicular cliffs.

Port Albany, Cape York, was reached on Sept. 1. A number of Australian birds, among them *Megapodius*, were found by Mr. Moseley in "Booby Island," which is now no longer used as a post-office, as the steamers touch at Somerset. The most interesting among the birds of North Australia is the Australian Bird of Paradise (*Ptilornis magnifica*). The landscape, owing to the grey green of the *Eucalyptus*, is monotonous in comparison with the rich green foliage of the Polynesian islands. Termites are very abundant; their mounds being twelve feet high. The queen is not so large as that of the West Indian kind, and did not inhabit any particular royal boudoir, but was to be found here and there in one of the corridors. Of the aborigines only fifteen were alive—"the rest of the members of the stock, who once lived at Cape York, have all been quieted (pacificirt) to death." *

On Sept. 8 the *Challenger* left Somerset and arrived at Dobbo,† an important trading town in the island of Wamma, one of the Aru Islands, on the 16th. From Dobbo the ship proceeded to the Ké group, then to the island of Banda, and afterwards to Amboina, which was reached on October 4. From Amboina the voyage was resumed as far as Ternate, thence into the Celebes sea. On the 26th the *Challenger* proceeded into the Sulu sea, and reached Manila, *viâ* the "Eastern passage," on Nov. 4. A great prize, in the shape of a specimen of the cephalopod *Spirula*, was found in the trawl off the coast of Banda, in the Moluccas, which had evidently passed through the digestive tract of some large fish, probably a *Macrurus*. It is probable that the animal lives in a medium depth of from 300 to 400 fathoms. This is the fourth specimen which has been obtained.‡ An *Amphioxus* was found

* *Vide* a letter from Von Willemoes-Suhm to Professor Siebold (Ibid. Bd. xxvi.), dated Yokohama, Japan, May 1875.

† A picture of this town, as it appears in the trading season, is given in Wallace's "Malay Archipelago," vol. ii.

‡ Von Willemoes-Suhm. Ibid. The history of the other three specimens

in the shallow Aru sea, as well as in the shallow water by Cape York. Commander Chimmo's observations, made in and around the Malay Archipelago, were in the main confirmed. It appears that there is a singular succession of marine basins which are cut off by barriers of varying height from communication with the ocean. From Api to Raine Island the *Challenger* had passed through a breach in the great barrier reef, not far from the entrance of Torres Strait. A sea was then traversed for an extent of 1,400 miles, having a bottom of red clay, and which was included within a broken barrier consisting of Australia to the west, the Louisiade Archipelago, Solomon Islands, and a small part of New Guinea to the north, the New Hebrides to the east, and New Caledonia and a line of reefs connecting this with Australia, to the south. This, the "Melanesian Sea," had no free communication with the outer ocean to a greater depth than 1,300 fathoms. In the Arafura Sea, between Somerset Island and the Aru islands, there is no greater depth than 50 fathoms—the average being from 25 to 30 fathoms; with a bottom of greenish mud, due to the great rivers of New Guinea. The Banda Sea—900 fathoms being the lowest limit of the barrier enclosing it—has for its boundaries Taliabo, Buru, and Ceram on the north, on the east the Aru islands, Timor and Serwatty islands on the south, and Celebes and the shoals of the Flores Seas on the west. The Molucca Passage communicates freely with the outer ocean, and also, by a passage of 700 fathoms depth, with the Celebes Sea. The Sulu Sea was the fourth of the series of marine basins through which the *Challenger* passed in succession from Api to Manila.*

The ship arrived at Hong-Kong from Manila on Nov. 16. Here Captain Nares received a message recalling him home, and giving him the command of the Arctic Expedition, his place being filled by the appointment of Captain Thomson, who was at that time in command of H.M.S. *Modeste*, on the China station. After a thorough refit the *Challenger* again put to sea on Jan. 6, 1875, and passing along the west coast of Luzon, anchored off Manila on Jan. 11. On the 8th, being in the centre of the China Sea, serial temperatures at every 50 fathoms to 400 fathoms, and every 100 fathoms to 1,000 fathoms were taken. At 900 fathoms the temperature was 36°, and this was maintained to the bottom—2,100

are as follows:—1. Sent to England from New Zealand by Mr. Earl, and figured by Mrs. Gray. 2. Sent to France by Péron, and described by De Blainville. 3. Found on the surface of the Sulu Sea, on the voyage of H.M.S. *Samarang*, and described by Professor Owen.

† "Nature," vol. xi. p. 288.

fathoms. This mainly agreed with results obtained by Commander Chimmo, tending to prove that the China Sea is cut off from the Antarctic basin by a submarine rampart, the top of which is between 800 and 900 fathoms beneath the surface.

The news which the *Challenger* brought, that Prince Alfonso had been chosen king by the Spanish people, created but little interest at Manila, the form of Government being indifferent to the officials there so long as they were allowed to retain their posts. On the 11th the ship left Manila, and after passing through the San Bernardino Strait, and the narrows among the islands, anchored off the island of Zebu on the 18th. This was chosen as a halting-place in order that specimens of the lovely *Euplectella* might be obtained. These sponges, called "regarderas" by the Spaniards, seem to be very local, being all obtained from one spot off the island of Mactan, close to Zebu. They are found at a depth of 100 fathoms, and are fished for by the natives by means of a most ingenious kind of dredge made of two slips of bamboo meeting at an angle and armed with large hooks, which is dragged slowly over the mud. The investing layer of sarcode was not so thick as would be expected, scarcely masking the form of the delicate architecture of the spicules, being thus not nearly so spongy as another species of the same genus dredged off the coast of Portugal.* Coal is found in Zebu, and, if labour could be found, would supply the whole Archipelago. On the 25th the volcanic island of Camiguin was visited, the capital of which, Catarman, was entirely destroyed by a volcano which sprung up close to it on May 1, 1871. Since then the earthquakes, which were once frequent, have ceased, and the mountain has increased to a height of 2,000 feet. A sounding was obtained close to the island in 185 fathoms, and the bottom temperature obtained—57°—shows that the volcano did not affect the temperature of the sea. On Feb. 22 the *Challenger*, having, after leaving Zebu, coaled at Malinipa Island, anchored in Humboldt Bay,† New Guinea. "That there is a great future for this vast island," well observes Capt. Davis, "there can be no doubt, situated as it is so near our own colonies in Australia, and producing so much; for that this country, with its accessible sea coast, should remain unproductive, as far as the great family of man is concerned, and entirely closed to commerce, is an anomaly in this nineteenth century that cannot well be understood." ‡ No exploration was made of any part of New Guinea, because of the doubtful attitude of the inhabitants.

* "Geographical Magazine," Dec. 1875, p. 359; and a letter in the "Times" of April 30, 1875.

† Visited by Capt. D'Urville, in the *Astrolabe*, in 1827.

‡ "Geographical Magazine," loc. cit.

On March 3 the ship arrived at Admiralty Island, and a bay on the north-west coast was carefully surveyed, and called Nares' Bay, after the late captain. The natives seemed to be superior in every respect to those of Humboldt Bay, and traded with great eagerness.

On March 23, nearly midway between the most southward of the Ladrones and the north-easternmost of the Pellew group, in lat. 11°23′ N. and long. 143°17′ E., the two deepest soundings on record were made, being 4,575 and 4,475 fathoms, the bottom being red clay. The pressure, however, was so great, being five or six tons to the square inch, that three out of four Miller-Casella thermometers succumbed. One, however, fortunately returned in safety, registering a temperature (corrected for pressure) of 34°5, so that at that spot there is a layer of water of that uniform temperature for 3,075 fathoms. As a proof how perpendicular the sinkers descended, particles of the mercury from the broken thermometers were found embedded in the red clay brought up on the tube. A somewhat deeper sounding, viz. of 4,643 and 4,655 fathoms, is said to have been obtained by the United States vessel *Tuscarora* off the east coast of Japan, but the corroborative evidence of a bottom specimen was not obtained. On April 11 the expedition reached Yokohama. On June 16 the *Challenger* left Yokohama, and, after having worked out twenty-four stations in an easterly direction, took a southward course, and reached Honolulu, in the Sandwich group, on July 27. From June 17 up to July 24 the ship was followed by albatrosses, varying in number from fourteen to twenty daily. On July 2 a new species of *Hyalonema* came up in the trawl, which was remarkable for the absence of the zoophyte *Palythoa*, which usually lives as a "commensal" in company with this kind of sponge. The bottom throughout this portion of the cruise consisted of "red clay." On the evening of July 27 the *Challenger* anchored in the harbour of Honolulu.

On the voyage from Hawaii (where the *Challenger* was in August) to Tahiti, the scientific staff of the ship sustained a most severe loss in the death, on Sept. 13, from erysipelas, of Dr. von Willemoes-Suhm. Of him Professor Wyville Thomson says that he was "perfectly certain, had he lived, to have achieved a distinguished position in his profession, and I look upon his untimely death as a serious loss, not only to the expedition in which he took so important a part, but also to the younger generation of scientific men, among whom he was steadily preparing himself to become a leader." *

The last news received from the *Challenger* is to the effect that the ship had safely arrived on Nov. 19 at Valparaiso.

* "Nature," Dec. 2, 1875.

There is, unfortunately, only space for the barest *résumé* of the new facts in zoology and physics established during the voyage. With regard to land-crabs, it had been previously asserted by Bell, contrary to the supposition of Vaughan Thompson, that the young of these crustaceans, like those of *Astacus fluviatilis*, have the same form as their parents on first coming into the world. Now Dr. Suhm, on examining a string of "berry-like pedunculated eggs" of a land-crab belonging to the genus *Cardisoma* (species doubtful), caught in the bay of Porto Praya (Island of San Jago), found some containing young ones which "*were not like their* mother, but Zoëas." The Zoëa of *Cardisoma* leaves the eggs in a somewhat more advanced state than that of *Carcinus mœnas*, representing a middle stage between the larva of the latter which has just left the egg and the one after its first moult (see figs. by Spence Bate in "Phil. Trans." 1858, Pl. xl.). It is probable that the Zoëas leave the mother, and lead a pelægic life until they have undergone all their metamorphoses.* At the Cape of Good Hope Mr. Moseley succeeded in obtaining thirty specimens of *Peripatus capensis*, an animal whose exact position in the zoological scale has been hitherto somewhat doubtful. The most recent information concerning it has been given by Grube in the zoological series of the *Novara* expedition. Nearly all the specimens were found at Wynberg, between Simon's Bay and Cape Town. *Peripatus* seems to be very local in distribution, affecting damp places, feeding on rotten willow-wood, aud being nocturnal in habit. When irritated it shoots out with great suddenness, from papillæ about the mouth, a "viscid tenacious fluid, which forms a meshwork of fine threads." This is not irritant, but as sticky as bird-lime, so that flies are held fast by it. Professor Gegenbaur holds that the position of this animal among the worms is not certain, but that, at any rate, it connects ringed worms with Arthropods and flat worms. With this Mr. Moseley agrees; adding, however, that it is probably an intermediate form linking the ringed and flat worms together with the "tracheata," and may well be placed among Professor Haeckel's "Protracheata."† Mr. Gulliver, when with the Transit of Venus Expedition in 1874, looked carefully for *Peripatus* on Rodriguez Island, but did not find any specimen. On June 15, 1875, halfway between Vries Island, Oosima, and Cape Sagami, there was found adhering to the trawl a specimen of a new family of Nemertines, for which Mr. Moseley proposes the name *Pelagonemertes Rollestoni*. In the flattened form of its body, and in

* "Trans. Linn. Soc." 2nd series. Zoology, p. 46, and Pl. XI. fig. 1.

† "Proc. Royal Soc." vol. xxiii. p. 344, and "Phil. Trans." vol. 164, p. 757, 1874.

having a dendrocœlous intestine, it resembles the Planarins, but in all essential structures it is most distinctly a Nemertine.*

With regard to *Bathybius*, the reputed animal organism, first described by Professor Huxley in 1868,† the organic nature of which has been denied by Dr. Wallich,‡ it seems highly probable that the latter observer is right, for Professor Huxley, *à propos* of a letter from Yeddo addressed to him by Professor Wyville Thomson ("Nature," Aug. 19, 1875), states of Bathybius that it is "seriously suspected that the thing to which I gave that name is little more than sulphate of lime, precipitated in a flocculent state from the sea-water by the strong alcohol in which the specimens of the deep-sea soundings which I examined were preserved." "The strange thing is," writes Professor Thomson, "that this inorganic precipitate is scarcely to be distinguished from precipitated albumen, and it resembles, perhaps even more closely, the proliferous pellicle on the surface of a putrescent infusion (except in the absence of all moving particles), colouring irregularly but very fully with carmine, running into patches with defined edges, and in every way comporting itself like an organic thing." In the same letter it is stated that the "pseudopodia" of *Globigerina*, sought for in vain at the beginning of the cruise, have been at last, and frequently, seen. If a specimen be immediately transferred from the tow-net to some fresh sea-water, and be examined with a high power, the "sarcodic contents of the chambers may be seen to exude gradually through the pores of the shell and spread out until they form a gelatinous fringe or border round the shell, filling up the spaces among the roots of the spines and rising up a little way along their length."

Coccospheres and Rhabdospheres live abundantly on the surface, especially in the warmer seas. If a bucket of water be allowed to stand over night with a few pieces of thread in it, many examples will be found attached. An unfailing supply, too, was found in the stomachs of *Salpæ*. From very numerous observations it may be stated as an axiom, that whenever the depth increases from about 2,200 to 2,600 fathoms the modern chalk formation of the Atlantic and of other oceans passes into a clay—the "red clay"—through an intermediate stage, termed by Professor Wyville Thomson the "grey ooze."

Concerning this red clay, which takes such an important

* "Ann. and Mag. Nat. Hist." 4th series, March and Dec. 1875, and Pl. XI.

† *On some Organisms at Great Depths in the Atlantic,* "Quart. Journ. Micr. Science," Oct. 1868, p. 210.

‡ *On the Vital Functions of the Deep Sea Protozoa,* "Monthly Micr. Journ." Jan. 1869, p. 38; and *On the True Nature of the so-called "Bathybius."*—"Ann. Mag. Nat. Hist." Nov. 1875, p. 122.

share in forming the floor of the ocean, there seems to be no doubt, according to Professor Wyville Thomson, that it "is essentially the insoluble residue, the *ash*, as it were, of the calcareous organisms which form the *Globigerina* ooze;" after the carbonate of lime, which forms about 98 per cent. of this ooze, has been by some means removed. A sample of such ooze was washed by Mr. Buchanan, and subjected to the action of a very dilute acid, and he found that there remained, after the carbonate of lime had been removed, about one per cent. of a reddish mud, consisting of silica, alumina, and red oxide of iron. Now, as all sea-water contains a certain proportion of free carbonic acid, which is greatest at the greatest depths,* it is very probable that this is the agent by which the solution of calcareous matter has been brought about. It is evident, then, that *clay*, which has hitherto been regarded as essentially the product of the disintegration of older rocks, may, under certain circumstances, be an organic formation, like chalk.† Professor Wyville Thomson, having formerly held the contrary, has been at length led to believe, from the results of Mr. Murray's explorations of the surface and sub-surface water, that the *Globigerinæ* pass their whole lives in the superjacent water, only subsiding to the bottom when they are dead. Dr. Carpenter believes that "the truth lies between two extreme views," and that the animals sink to the bottom *whilst yet living*, in consequence of the increasing thickness of their shells, and not only continue to *live* on the sea-bed, but also to *multiply* there.‡

Mr. Buchanan made some observations on sea-water ice, which is known, when melted, to be unfit to drink. Fragments of pack-ice, and also of some ice which had formed in a bucket of sea-water on board, were submitted to various tests, and it was found that the salt is not contained in it only in the form of mechanically enclosed brine, but exists in the solid form, either as a single crystalline substance or as a mixture of ice and salt-crystals. The sea-water ice which had crystallised in hexagonal planes, as common salt does when separating from solutions at temperatures below 0° C., may possibly have some analogy to the isomorphous mixture occurring amongst minerals.§

* There is, moreover, according to Mr. Sorby, an increase of solvent power for carbonate of lime possessed by water under greatly augmented pressure.

† See *Preliminary Notes on the Nature of the Sea Bottom procured by the Soundings of H.M.S. Challenger, during her Cruise in the Southern Sea, in the early part of the year* 1874.—"Proc. Royal Soc." vol. xxiii. pp. 44, *et seq.*

‡ Remarks on the preceding paper.—Ibid. p. 234.

§ *Some Observations on Sea-Water Ice.* "Proc. Royal Soc." vol. xxii. p. 431.

For a most able *résumé* of the fresh facts established by the expedition on which the *Challenger* was despatched the writer is indebted to a lecture delivered by Professor Huxley at the Royal Institution.* The existence of a fine clay in the deeper parts of the Mediterranean had been already known, but no one suspected the existence of deposits of barren clay in the middle of the great calcareo-silicious zone in the open ocean. The dredgings of the *Challenger* have further established that certain ocean-valleys contain thick deposits of finely divided "red clay," the nature of which has been already described, *e.g.* between Teneriffe and St. Thomas, at the depth of about 3,000 feet. If it be, as Professor Wyville Thomson suggests, that the deposits of "red clay" represent remains of myriads of marine organisms, not only silicious and calcareous, but also argillaceous deposits may be formed by long-continued vital agency. Further, it follows that rocks of almost any mineral composition may be indirectly or directly generated by living organisms. It is certain that—

(*a*) Beyond certain depths the calcareous organisms which must fall over the area covered by the ocean disappear, and their place is taken by a fine red clay.

(*b*) When ordinary *Globigerina* ooze has its calcareous matter removed, a residue of fine red clay remains.

Supposing that the earth were covered uniformly with water to a depth of 2,000 fathoms (or about two miles), the merely tidal and current movements already existing would be insufficient to cause any important degradation of the crust, so that there would scarcely be any sedimentary deposits. Let, then, Diatoms, *Foraminifera*, *Radiolaria*, and Sponges be introduced; there would then be formed the silicious Pole-caps, and the intermediate zone, which might accumulate till beds of chalk were formed many thousand feet in thickness. The chalk might next be converted into limestone, and so all traces of its origin would be effaced. Next, let parts of the area be depressed to 1,800 feet and other parts be raised to within 1,000 feet; then, judging by what we now know, the former might be replaced by red clay and the latter by greensand.† The clay might be metamorphosed into shales, slates, &c., and the greensand into minerals into the composition of which silica, alumina, iron, and potash enter; so that the imaginary world would be covered with rocks all due to an organic origin, but of

* *On the Recent Work of the Challenger Expedition, and its Bearings on Geological Problems.* Friday evening, Jan. 29, 1875.—"Proc. Royal Inst." vol. vii. part 5 (No. 62), April 1875.

† A *modern* greensand in course of formation was found by Professor Wyville Thomson in the region of the Agulhas current.

which all traces were obliterated. The application of this hypothesis might reconcile the discrepancy between biological theory and geological fact.

1. There is evidence that the present species of animals and plants have arisen by gradual modification of pre-existing species.

2. There is also evidence that, geologically speaking, this process has extended over a longer period than that of the fossiliferous rocks.

3. Nevertheless, beneath the latter there lies a great thickness of formations almost entirely azoic.

Such researches consequently lend support to the views of those geologists who find the explanation of the past history of the earth in the present operations of Nature—views held half a century ago by Sir H. de la Beche, and developed by Sir C. Lyell, the able supporter of the Uniformitarianism enunciated by Hutton.

Want of space and pressure of time permit of no more than a passing allusion to the interesting controversy which has been going on for several months between Dr. Carpenter and Mr. Croll, of the Scottish Geological Survey, on the subject of "The Crucial Test of the *Challenger*." * Mr. Croll's position seems to be that all the great movements of ocean-water, deep as well as superficial, depend upon the action of winds upon its surface. Though admitting that the Polar water finds its way along the floor of the great ocean basin into the Equatorial area, he affirms that this is merely the reflux of the current which has been driven into the Polar basin by the agency of winds. Dr. Carpenter admits that the *current* movements of *surface*-water are, for the most part, produced by winds, but all these belong to a *horizontal circulation which tends to complete itself*, a surface indraught being produced when a surface outflow is kept up, but that the *deep* movements are the result of a *vertical circulation*, maintained by the continuance of a disturbed equilibrium between Polar and Equatorial columns, occasioned by the surface action of Polar cold and Equatorial heat. It seems, too, that Dr. Carpenter's views as regards a vertical circulation are in accordance with the opinions of Emil Lenz, a physicist who accompanied Kotzebue in his second voyage of circumnavigation in 1823–26. As, however, the controversy does not yet appear to be at an end, and as the subject properly concerns a physicist, any expression of opinion on the part of the writer would be of but little value.

* See "Nature," vol. ix. p. 423, and other papers in the same periodical, as well as a series extending over several numbers of the "Philosophical Magazine."

It is to be hoped that, before the next number of this Review appears, we shall have welcomed back the victors with rejoicing, tempered, however, with sorrow that one out of this brave little band has been left behind in the lone ocean—a congenial resting-place, it may be, seeing that it was the scene of his late labours. "Whom the gods love, die young."

EXPLANATION OF PLATE CXXIX.*

"Es giebt ein vollendetes organisches Leben im unsichtbar kleinen Raume, welches die Grösse des Grossen in der Natur unabsehbar erhebt."—'Ehrenberg, *Die Infusionsthierchen.*" 1838.

FIG. 1. *Globigerina*, with the radiating processes entire. From the *Challenger* soundings. After Pl. I. vol. xxiii. "Proc. Royal Soc." It is evident that when this figure was drawn the investing envelope of sarcode, mentioned by Professor Wyville Thomson in his letter to Professor Huxley ("Nature," Aug. 19, 1875), had not yet been discovered.

FIG. 2. A "Cyatholith," from the Atlantic mud, magnified 1,200 diameters. After fig. 4, *c*, *i*, Pl. IV. "Quart. Journ. Micr. Sci.," New Series, 1868.

FIG. 3. A "Cyatholith," from the chalk of Sussex, magnified to a similar degree. Ibid. fig. 5, *b*, *c*.

FIG. 4. A cluster of "Coccospheres," of the *loose* type, about $\frac{1}{760}$th inch in diameter, from the Atlantic mud. Ibid. fig. 7, *d*, and p. 209.

FIGS. 5 and 6. "Rhabdospheres" (Murray), from the *Challenger* soundings. After figs. 3 and 4, Pl. III. vol. xxiii. "Proc. Royal Soc."

FIG. 7. "The new Diatom." Found a little to the North of the Heard Islands. Ibid. fig. 5.

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* The woodcuts have been kindly lent by Messrs. Macmillan & Co.

WATER SUPPLY AND PUBLIC HEALTH.

By W. TOPLEY, F.G.S.,

Assoc. Inst. C.E. Geological Survey of England and Wales.

IN the year 1868 a Commission was appointed to inquire into the best means of preventing the pollution of rivers, with special reference to the disposal of sewage, to questions relating to water supply, and the public health generally. Five Reports have appeared during the last seven years; these dealt with the pollution of certain selected rivers, the question of water supply being only incidentally referred to. In the sixth and final Report the Commissioners devote their attention exclusively to the "Domestic Water Supply of Great Britain." *

The names of Dr. Frankland and Mr. J. C. Morton are sufficient guarantee for the accuracy of the information given in the Report. But in this complicated question names alone, however eminent, are not sufficient to stamp with unquestioned authority all opinions and inferences; and we do not doubt that some conclusions contained in the Report will be stoutly denied by authorities equally eminent. That the work, so far as *data* are concerned, has been thoroughly done, will be evident from the fact that more than 2,000 analyses of water have been made. These have all been made on a definite plan, and are arranged as systematically as could conveniently be done. The Commissioners state that "water has been followed through the complete cycle of its migrations; it has been caught as it descended from the clouds soon after its condensation from colourless and invisible vapour, collected as it flowed in streams after washing the surfaces upon which it fell, examined after it had penetrated to various depths through

* "Sixth Report of the Commissioners appointed in 1868 to inquire into the Pollution of Rivers." [By Dr. E. Frankland and J. C. Morton.] Folio. Lond. 1874. Pp. xi. 525; Maps and Plans. (Although dated 1874, the Report was not issued till late in 1875.)

different geological strata, and finally it has been investigated after it had become part of the great mass of the ocean."

The Report is accompanied by four maps and several plans and diagrams. One map, by Mr. C. J. Symons, shows the rainfall of the British Isles. Another is geological; this is prepared by Mr. E. Best, chiefly from the maps of the Geological Survey.

In 1867 a Royal Commission was appointed to inquire into the question of water supply, with especial reference to the metropolis; this Commission issued its Report in 1869. The two Commissions thus traversed to a great extent the same ground, and their Reports will serve as a mine of information on this subject for years to come.

During 1844 and 1845 a series of Reports were issued on the "Health of Towns," which deserve to be better known than they are; but, unfortunately, it is often the fate of "blue books" to be forgotten soon after publication. These Reports are worthy of attention even now for the topographical information which they frequently contain; still more are they worthy of study as denoting the immense advance of sanitary measures during the last thirty years. Bad as matters now are in many large towns, they were infinitely worse then; and sanitary reformers may well take heart at the contrast. Water supply was then one of the most important subjects which engaged the attention of the reporters. Recourse was subsequently often had to neighbouring rivers or streams for a regular supply under efficient control, and for a time all went well. But the towns increased in size, systematic sewerage was introduced, and the rivers were rapidly fouled. And now the problem of how to supply pure water to many overcrowded towns is again awaiting solution.

But, in dealing with this question, it is important to bear in mind that the problem is no longer limited to the larger towns and the great centres of industry. Here there are wealth and public spirit, and sooner or later such places will provide for themselves. But, unless carefully watched, such provision is often made at the expense of smaller towns and villages. One great point in future legislation should be to secure an adequate supply of water for entire districts, especially for those situated within the drainage area from which the supply is drawn.

A study of a good geological map shows that the old village settlements cluster along water-bearing strata; generally they are on or near the edge of porous beds, into which the rain partly sinks as it falls, to be again thrown out as springs at the edge of an impervious bed. There were doubtless other reasons for selecting these sites; land which absorbs water has a dry soil, suited alike for dwellings and arable culture, in days when draining was unknown. The neighbouring heavier lands were then generally thickly wooded.

Many interesting questions are associated with this inquiry. We can often trace out the earliest settlements of a district along the outcrops of certain strata yielding the best soil and an abundant supply of water; that they are the earliest settlements is evident from an examination of the relations of their parish or township boundaries to the neighbouring hill ranges, and sometimes also from a study of their names.*

In the course of thirteen centuries many changes have been wrought on the physical features of the country. Forests have all but disappeared from the plains and the hill slopes, fens and morasses have been drained. As a consequence partly of these changes many springs which once ran strongly now give but a short and inconstant supply; streams are lessened in flow, and are often nearly dried up in summer. The climate has become drier; but whether the actual amount of rainfall has diminished during this period, and if so how far this change is due to man's influence, are questions at present undecided.

One result of modern agricultural drainage has been to bring the rainwater quickly down into the brooks, and hence the rivers have risen into flood more quickly than before. That the water does descend more quickly after rain than it did some years back, is a matter of common observation amongst anglers and farmers; they also remark that the rivers more rapidly regain their ordinary level than they used to do. These observers, in common with nearly all agricultural authors, explain these facts by the prevalence of drainage. Mr. R. Rawlinson and the Rev. J. C. Clutterbuck, however, deny this; † and the former states that the object of drainage being to carry the water down through the soil, the result must be to retard the flow of water.

But whatever may be the result of under-draining cultivated lands, there can be no doubt of the effect of trenching the upland pastures, moors, and peat bogs, amongst which most of the northern rivers take their rise. Peat acts like a sponge in absorbing the rainfall; the surface of some bogs often rise very considerably when distended by water, and at times when over-strained the surface bursts and considerable damage ensues. But this is only the case with what are termed "flowes" or shaking bogs, which generally occur at low levels; and it rarely happens with the peat bogs of hill districts.

* I have discussed this question in a paper "On the Relation of the Parish Boundaries in the South-East of England to great Physical Features, particularly to the Chalk Escarpment."—"Journ. Anthrop. Inst." vol. iii. p. 32, 1873.)

† See their letters in the "Times" for November 16 and 19, 1875.

These high peat bogs are reservoirs of water, which they collect in winter and yield gradually in summer. They generally lie at too high a level to be cultivated for grass or corn, but they are capable of some improvement as rough upland pasture. This improvement is often secured by deeply trenching the bogs in various directions; the water then drains off, the soil becomes drier and affords feed for sheep. This process is largely going on, and if continued will, in the course of only a few years, make its results seriously felt on the summer and autumn flow of the rivers in the north-east of England. Such results will be less felt, indeed may be comparatively unimportant, in most rivers on the western side of the great central watershed of England; for there the rainfall is much greater, and the periods of drought are shorter. But on the eastern side of the watershed it is simply equivalent to destroying a large number of natural "compensation reservoirs," which at present serve to diminish the winter floods and to augment the summer flow. The additional value conferred on the uninhabited upland moors is but small; the loss to the populous cultivated lowlands is immense.

The late floods have once more made evident the importance of storing the surplus rainfall for summer use. This, with more embankments and fewer weirs, will lessen the floods. Some day probably all this will be done, but at enormous expense. Meanwhile it might be well to see to the preservation of those reservoirs which Nature has herself provided.

A supply of water and the disposal of sewage are two of the most difficult problems of modern times; whilst either, taken alone, would in many cases be comparatively easy. The readiest mode of getting rid of sewage is to pour it into the rivers and streams; but this fouls the natural source of water supply. Pollution of rivers by such means will in time be largely checked; we shall some day discover that sewage is too valuable to be thrown away. But even if the utilisation of sewage should not prove actually profitable, it will be found advantageous to spend public money on this, and thus to prevent the fouling of rivers to the extent now practised and each year increasing. By these and other means the pollution of rivers may be greatly reduced, but it can never be wholly prevented; and it is useless to look for a supply of water for drinking purposes from rivers which traverse manufacturing or populous districts.

"Doctors disagree" in many things of great moment to the public health, but in none is this disagreement more to be regretted than in questions relating to water supply. Complete accord as to opinions and inferences is not to be expected, but we may certainly hope for more agreement as to facts and as to the mode in which these facts are published. Carefully prepared official Reports on the quality of the metropolitan waters are frequently published by different chemists, but they are made on different plans, and cannot be readily compared with each other.

In old analyses of water the imperial gallon was taken as the standard measure, and the results were given in grains per gallon. As the gallon contains 70,000 grains of water, this is the same as giving parts per 70,000. In 1864 Dr. A. W. Hofmann, then of the Royal School of Mines, was appointed to report on the metropolitan waters to the Registrar-General. He introduced the system of giving the results of analysis in parts per 100,000—a system which has been continued by his successor, Dr. E. Frankland. The analyses for the River Pollution and Water Supply Commissions are given in this form. The imperial gallon is, however, still retained by most chemists; amongst others, by Dr. Letheby, Dr. Voelcker, and Professor Wanklyn.*

This want of agreement is frequently a matter of great inconvenience. As regards the general constituents of water it is less so, because for detailed analyses the standard is generally stated. But where "hardness" alone is spoken of, it is often doubtful which scale is used. This is a point of importance, because the term is in frequent use, and we often read of the hardness of certain waters when no other chemical facts regarding them is stated or perhaps known.

Those who adhere to the gallon as the standard generally describe hardness by what is known as Dr. Clark's scale, which is the same as grains per gallon; a hardness of 7° denoting seven grains of carbonate of lime or magnesia (or their equivalent as sulphates) in a gallon of water.† Dr. Frankland gives hardness in parts per 100,000; so that a hardness of 10° in his analysis is only equal to 7° in Clark's scale. In comparing this and other analytical details there is a simple rule:—to convert parts per 100,000 into grains per gallon (or parts per 70,000), multiply by 7 and divide by 10; to convert grains per gallon into parts per 100,000, multiply by 10 and divide by 7.

Chemists are not unanimous as to the best means to be

* Professor Wanklyn also sometimes states his results in parts per million (or millegrammes per litre). A simple division by 10 converts this into Dr. Frankland's scale of parts per 100,000.

† A further confusion as regards hardness has been introduced by a slight modification of Dr. Clark's method of expressing his result. Even distilled water requires to dissolve some soap before a lather is produced; the quantity required is about one grain to a gallon of water. This by some chemists is added to the result in order to state the actual soap-absorbing power of the water. According to this method, a hardness of 8° is equal to seven grains of carbonate of lime per gallon.

adopted in determining the constituents of organic origin contained in water, and the two Commissions are not at one as to the inferences to be drawn from analyses, even when correctly obtained.

The older analyses of water refer only to "organic matter," without reference to its character; but this really gives us little information of value. One water containing organic matter may be comparatively harmless; whilst another, containing a far smaller quantity, may be highly dangerous. It depends altogether upon the source and nature of the organic matter. This is a very complicated question and cannot be briefly discussed with advantage; but we will note the chief points relied upon by chemists, especially Dr. Frankland. It is necessary to know the precise amount of nitrogen and of carbon (other than that existing as carbonic acid or carbonates); the relative proportion of organic carbon and organic nitrogen gives a clue to the character of the organic matter—whether of vegetable or animal origin; the smaller the absolute amount of nitrogen, and the higher the proportion of carbon to nitrogen, the less the danger of animal matter.

But here a difficulty occurs; during the oxidation of vegetable organic matter the proportion of carbon to nitrogen *lessens*, whilst during the oxidation of animal organic matter the proportion of carbon to nitrogen *increases*. We are thus often thrown back on a knowledge of the source of the water to enable us rightly to interpret the chemical analyses. But, apart from this knowledge, if the absolute amount of organic nitrogen be high, we may reasonably regard the water with suspicion.

Nitrogenous organic matter or "albuminoid ammonia" undergoes putrefaction, and is converted into "free ammonia." The presence of much free ammonia in the water of shallow wells denotes very recent contamination with animal matter. A large proportion of nitrates or nitrites shows that the pollution is more remote, the ammonia having become oxidized into nitrates. But in the case of deep wells the presence of a little ammonia need not denote recent pollution, it may have been formed by the deoxidation of nitrates. But of this at least we may be certain—if the amount of nitrates, nitrites, or ammonia be large, the animal pollution of the water, recent or remote, must have been considerable.

Water from deep wells in the chalk often contains variable proportions (though rarely large) of ammonia and nitrates. One curious result of the careful tabulation of the analyses in the River Pollution Report is this:—The wells sunk through London clay into the chalk yield water of less hardness and with more ammonia than those sunk into the chalk when that rock forms the surface of the ground; nitrates are frequently absent.

Wells on bare chalk give water which always contains nitrates, but from which ammonia is frequently absent. In fact, water from chalk when under London clay appears somewhat to resemble the water from the lowest Tertiary strata, which lie between the chalk and London clay; probably the chalk in such situations is partly fed by water draining downwards from the overlying sands.

Chlorine exists in water chiefly as common salt (chloride of sodium). Water which has been much polluted by sewage always contains this substance in considerable quantity; it is therefore a guide in reading the history of waters. But sewage is not the only source of chlorine. The rain and the air of maritime districts are often charged with sea spray, and during heavy storms salt has thus been carried far inland. After the severe storm of Jan. 1839 salt was found on the trees near Huddersfield, sixty miles from the sea. In Nov. 1703 salt was carried into Sussex, more than ten miles from the coast, making the leaves and branches quite white. Salt will also occur in water after infiltration through strata naturally containing it.

Much discussion has arisen as to the phrase "previous sewage or animal contamination," in which Dr. Frankland sums up the history and quality of various waters; some chemists objecting to the phrase, as sensational or wholly delusive. The meaning of it is this:—An estimate is made of the total amount of combined nitrogen contained in solution in 100,000 parts of average London sewage. This amount varies at different times; in 1869 it was 7·06 parts, in 1857 it was 8·363. For simplicity the number 10 has been assumed. But rain-water always contains nitrogen in these forms; numerous analyses give a mean of ·032 per 100,000 parts as the amount of nitrogen present in rain water as ammonia, nitrates, or nitrites.

"After this number (·032) has been subtracted from the amount of nitrogen, in the forms of nitrates, nitrites, and ammonia, found in 100,000 parts of a potable water, the remainder, if any, represents the nitrogen derived from oxidized animal matters with which the water has been in contact. Thus a sample of water which contains, in the form of nitrates, nitrites, and ammonia, ·326 part of nitrogen in 100,000 parts, has obtained $\cdot 326 - \cdot 032 = \cdot 294$ part of that nitrogen from animal matters. Now this last amount of combined nitrogen is assumed to be contained in 2,940 parts of average London sewage, and hence such a sample of water is said to exhibit 2,940 parts of previous sewage or animal contamination in 100,000 parts; or in other words, 100,000 lbs. of the water contain the mineral residue of an amount of animal organic matter equal to that found in 2,900 lbs. of average London sewage." *

* River Pollution Commission, Sixth Report, p. 14.

The presence of much nitrogenous organic matter or free ammonia in water is held by all chemists to be a proof of contamination. Dr. Frankland regards the presence of nitrates in the same light. But Dr. Odling, Professor Wanklyn, and others, look upon nitrates as giving no certain information on the matter. It is true that they may be formed by the oxidation of ammonia, this having previously been formed by the fermentation of organic matter; but nitrates exist in deep well-water, to which it is hard to believe that sewage or its derivatives gain access; and they must also be formed in the soil, from the albuminoid parts of plants. On the other hand, living vegetable matter in water abstracts nitrates, and thus the amount of them may be no sufficient measure of its previous pollution.

Organic matters in water are hurtful because of the putrefaction and fermentation which they undergo. But when these changes have reached their final stage, and the organic nitrogen has been wholly converted into nitrates or nitrites, the water is not necessarily unwholesome from its mere chemical contents. The danger is that germs of disease—of typhoid fever and cholera, or the ova of intestinal worms—may remain unchanged, suspended, but not dissolved, in the water. This is especially to be feared in the case of shallow wells, and of streams into which sewage is poured. That typhoid fever and cholera are in this way propagated by drinking water has been proved repeatedly.

The extent to which sewage is oxidised and rendered harmless during its transport by rivers is a question of supreme importance; unfortunately it is also one on which authorities differ. Dr. Letheby believes that if ordinary sewage, such as that of London, "be mixed with twenty times its bulk of the ordinary river water and flows a dozen miles or so, there is not a particle of that sewage to be discovered by any chemical process." Other chemists incline to the same opinion, or at least believe that in most cases sewage is destroyed if carried in this way for long distances.* Dr. Frankland and Mr. Morton devote much attention to this question; and as the result of many analyses of several river waters, including that of the Thames, they believe that "there is no river in the United Kingdom long enough to secure the oxidation and destruction of *any* sewage which may be discharged into it, even at its source." Against these statements should be placed the admission of the Commissioners that the tributaries of the Thames are often more highly polluted than is the river itself at Hampton.

* See the Minutes of Evidence of the Water Supply Commission. The main points bearing on this question are recapitulated in the Commissioners' Report, pp. lxxv–lxxxvii.

As the river and its tributaries receive sewage at many intermediate points, the "previous sewage or animal contamination" at Hampton ought to be considerable; the fact that it is not seems to prove that much of the sewage has been destroyed. The Commissioners explain this apparent anomaly by the *subsidence* of suspended animal matter in quiet parts of the river. Sir B. Brodie and Dr. Lyon Playfair contend that sewage may not be (often certainly is not) *fully* oxidised even after long carriage in river water. Professor Wanklyn states that although urea is rapidly transformed into ammonia and carbonic acid, yet that albumenised matter is very persistent indeed. Mr. Simon puts the matter in the right light in his evidence before the Water Supply Commissioners. He said: "Water into which sewage has been discharged is, in relation to the matter now under consideration, an *experiment on the health of the population*, and I do not think that that experiment ought to be tried."

With regard to the maximum amount of organic matters which may be allowed in drinking water, the following statements will be useful. The River Pollution Commissioners believe that surface or river water should not contain more than 0·2 part of organic carbon, or ·03 part of organic nitrogen, in 100,000 parts. Spring and deep-well water should not contain more than 0·1 part of organic carbon, or ·03 part of organic nitrogen in 100,000 parts.

Professor Wanklyn states that efficiently filtered water may contain from 0·05 to nearly 0·10 parts of albuminoid ammonia per million; when the amount rises to 0·10, or exceeds it, there is defective filtration. "It is a matter of observation that diarrhœa is frequently prevalent in communities which drink such water." Free ammonia exceeding 0·08 part per million generally denotes recent contamination with urine. Much albuminoid ammonia, little free ammonia and little chlorine, denote vegetable contamination; such water is very injurious to health.*

Dr. Frankland and Mr. Morton sum up in the following table their observations on the relative value of waters: † —

Wholesome	1. Spring water (198)	Very palatable	
	2. Deep-well water (157)		
	3. Upland surface water (195)	Moderately palatable	
Suspicious	4. Stored rain water (8—of rain water, 73)		
	5. Surface water from cultivated land	(175)	Palatable
Dangerous	6. River water to which sewage gains access		
	7. Shallow-well water (420)		

* These observations are quoted from "Water Analysis," 3rd ed., 1874, pp. 37, 39.

† Pp. 129 and 425 of Sixth Report. I insert, within parentheses, the number of analyses on which the generalisations are based.

Classified according to softness, the waters stand thus :—

1. Rain water
2. Upland surface water
3. Surface water from cultivated land
4. Polluted river water
5. Spring water
6. Deep-well water
7. Shallow-well water

As regards the water supply of London, the Report just published will greatly strengthen the case of those who are discontented with our present supply—not only as to its quality, modes of distribution and control, but also as to its source. It is not pleasant to think of drinking water mixed with sewage, even although the sewage may enter the river miles away, and the water be most efficiently filtered before delivery. To many who read this Report, what may now be only a matter of taste will become a matter of conviction. The Commissioners unhesitatingly condemn the water supplied from the Thames and the Lea; that supplied by the Kent Waterworks Company to London, from deep wells in the chalk, is good water, its only fault being a high degree of hardness. The New River Company partly derives its supply from springs and wells in the chalk. According to this Report the amount thus obtained is relatively too small to affect the general quality of the water; but absolutely the supply so obtained is considerable, nearly, 12,000,000 gallons per day.

Chalk water is always hard, chiefly from the presence of bicarbonate of lime. Dr. Clark invented a process by which this "temporary hardness" may be removed, which consists in mixing water saturated with lime with the water to be softened. The excess of carbonic acid, which holds the carbonate of lime in solution in the hard water, is then transferred to the free lime, and the whole falls down as insoluble carbonate of lime. Apart from the advantage of softening the water, this process has other recommendations; much organic matter is carried down with the precipitate, and the water will keep better in reservoirs than when in the natural state.

This process has been applied by Mr. Homersham to several waterworks. The water from the deep well at Plumstead was thus softened until the year 1861, when the works were bought by the Kent Company. None of the water now supplied to London is treated in this manner; but the process is used at Aylesbury, Canterbury, Caterham, and Tring, with the following results:—

	Hardness (parts per 100,000).	
	Before softening.	After softening.
Canterbury	26·3	4·9
Caterham	21·2	4·4
Tring	26·3	3·2

The hardness remaining after softening is due to the presence of sulphate of lime or sulphate of magnesia; this is termed "permanent hardness."

The plans which have been suggested for supplying London with water from distant sources, as from Wales and the Lake District, are those most frequently spoken of when dissatisfaction is expressed with regard to the present supply. This is not to be wondered at, for the schemes are bold and complete, the water promised is pure and abundant. Compared with these, the proposals for collecting spring and well water in various parts of the Thames basin, and bringing it by different routes to London, are "patchy" and tame; and yet perhaps in these characters lie their chief recommendation. Is it wise to trust for the supply of the metropolis to a single source, so far distant? Moreover, these distant sources may some day be wanted for the towns in the northern and midland districts.

There seems no necessity for going so far away, as plenty of good water can be obtained within the Thames basin itself. Indeed, no large city is better situated for supplying itself from within its own drainage area; although some—Paris, for instance—are more fortunate as regards deep wells. The average daily supply from the metropolitan companies during 1874 was 116,500,000 gallons, or 34·8 gallons per head; the maximum daily supply was in September, when it averaged 127,600,000 gallons, or 37·5 gallons per head. It is calculated that about one-third of the supply is wasted; one-third goes for trade purposes, flushing sewers, street watering, and fires; only one-third is used for purely domestic purposes.

The Commissioners on Water Supply estimated that it would be desirable to calculate on a future population of 5 millions, with a maximum summer consumption of 200 million gallons, or 40 gallons per head. The present excessive waste can be largely checked by the use of better fittings and more strict control; by such means the consumption in Liverpool has been reduced by Mr. Deacon from 28·89 to 16·47 per head, under constant service. But, to be on the safe side, we will take the figures as they stand. One reason for doing so is the *possibility* that the Thames valley may some day become a busy centre of mining industry. Geologists have long surmised that under the south-east of England there may lie fields of valuable coal measures, at a workable depth.* The Sub-

* Professor Prestwich has fully discussed this question in a former number of this Review (vol. xi. p. 225; 1872). The Metropolitan Board of Works are now putting down a boring at Crossness, which they intend to carry through the Gault. This is in search of water, but its results will

Wealden Boring has as yet thrown no certain light on this question. A decided negative result there will not prove the absence of coal measures under London, and other trials will one day be made in this direction. Wild as this surmise may seem, it is one worth considering as regards this question.

The following table exhibits the amount of spring water available within the Thames basin, including the supply available from wells of the existing companies :—

	Gallons per day.
Springs and wells of the New River Company	12,000,000
Wells of the Kent Waterworks Company	15,000,000
Chalk springs at Grays	10,000,000
,, ,, between Lewisham and Gravesend * . . .	43,000,000
,, ,, above London	30,000,000
Total from the chalk	110,000,000
Lower Greensand district of Leith Hill, Hinahead, &c.† .	20,000,000
Oolitic springs on the N. side of the Thames ‡	70,000,000
Total . .	200,000,000

These estimates are independent of the Bagshot district, and the lower greensand range from Redhill to Maidstone. The supply might be largely increased by additional wells sunk in the chalk, lower greensand, and oolites. A large quantity of water might be obtained by sinking near the Cotteswolds Hills, through the inferior oolite to the Midford sands; this, however, would probably rob the Severn of its spring water, which now flows out on the western side of the Cotteswolds.

It is thus evident that sufficient pure water can be obtained within a moderate distance of London ; sufficient even for the entire future consumption. To bring water from so many sources will certainly be expensive—perhaps as much so as the most costly scheme for far-distant sources. But the expense can be incurred gradually; the nearer sources may be taken first, those further off at later times, as the demand increases.

If the supply for domestic use can be separated from that for general purposes, an enormous saving of good water would

probably be valuable as regards our knowledge of the range of the older rocks. I may here remark that the description which I gave of the Sub-Wealden Boring in a previous number of this Review (vol. xiii. p. 399) requires some modification. A second boring, carried to a depth of 1,820 feet, has proved that the Kimeridge clay extends to 1,750 feet at least.

* Mr. Barlow estimated sixty millions from this source.

† Estimates of the yield of this district vary greatly. I take less than the lowest.

‡ River Pollution Commission, Sixth Report, p. 297. This is the estimated summer yield of only twelve springs.

result; as Thames water could be retained for the latter. The expense attendant upon this change would be very great, but it would probably be less than that required for many of the proposed schemes. Newcastle-on-Tyne and Gateshead are supplied from upland gathering grounds, supplemented when necessary with water pumped from the Tyne. A bill, to be submitted to Parliament during the coming session, provides powers to lay down duplicate mains, with the view of confining the Tyne water to the trade supply.

In the foregoing tabular statement the water is supposed to be obtained in three different ways: from wells, springs, and galleries driven along the water-bearing strata to intercept the subterranean flow of water. It is probable that this last plan will in time be extensively adopted. It was first systematically used by the late Mr. Easton, in supplying Ramsgate by headings driven near the chalk cliff, intercepting the water which before flowed to the sea. Mr. Barlow proposed to collect the chalk-water along the south side of the Thames in a similar way; and Mr. Lucas has recently proposed to use this method extensively for the supply of London. The headings driven from the sides or bottoms of wells to intercept the water-yielding fissures depend upon the same principle; such are now largely adopted. Water is thus *mined for*, along the lines at which it is most likely to occur.

The abstraction of so large an amount of spring water for the supply of London would sensibly diminish the summer flow of the streams. This would have to be restored by large compensation reservoirs in which to store the surplus rainfall. This might also in many cases be largely stored in the great natural underground reservoirs by carrying down shafts through the overlying impervious beds. If sewage can thus be absorbed and got rid of (as it has been on a large scale), the same may be done with surplus rainfall; but this should be done before the water is polluted. Drainage water on the tertiary beds is often turned into the underlying absorbent chalk; the porous beds of the lower greensand and oolites have been used in the same way.

The River Pollution Report refers to many subjects of great interest, in addition to those to which we have too briefly alluded. One of the most important divisions of the volume describes the water supply of provincial towns and villages. This brings out most forcibly how large, in all parts of the country, is the number of shallow wells which yield impure and even dangerous water. Many instances are given in which water fit only for irrigation is constantly used for drinking. A well in the market-place at Deal has a manure valley fully equal to that of average London sewage.

In another part of the Report many analyses are given of shallow wells in London, some of which have a manure value 150 per cent. greater than that of London sewage. Only two of the public wells are considered safe; they are the pump near the south-east entrance to Kensington Gardens, and a well at the Maritime Almshouses, Mile End Old Town. Of thirty-nine shallow public wells which existed within the City in 1866, only four are still open. Dr. S. Saunders has lately recommended that these be closed by order of the City authorities or of the Secretary of State.

This is a matter which has long been a public disgrace. There may be difficulties in the way of providing large towns with a plentiful supply of good water, but there should be none to prevent the closing of public wells which yield slow poison.

At the late Birmingham Sanitary Congress several speakers dwelt emphatically on the evils resulting from shallow wells; in Leeds and Bristol many such wells have been closed by the local authorities.

Rural districts are little if any better off than towns in this respect. Dr. Voelcker states that he analyses a dozen samples of bad water from country places for every one from towns, and that many drinking waters sent to him for analysis are fitted rather for irrigation. Mr. F. Sutton states that out of 429 samples of water taken from wells in the open country, 307 had to be condemned. On inquiry, it invariably occurred that either a dead well or a cesspool existed within a few yards of the well. He thinks it probable that waters suspected to be bad were sent for analysis. We cannot of course suppose that 75 per cent. of country wells are so bad as this.

But all this is an old and now familiar tale. Again and again we read in official reports of how the inhabitants of this or that house, village or town, are poisoned by the foul water of shallow wells. Perhaps in some exceptional cases a little general interest is excited by the statement; but the evils in the particular cases are more or less redressed, and matters go on elsewhere much as they were before. People still speak enthusiastically of the pleasant waters from their favourite wells, perhaps fed by the soakage from neighbouring cesspools or the drainage from adjacent burial-grounds. The connection between the source of pollution and the water drunk is not apparent, and little heed is given to scientific demonstration that such connection exists. And why should heed be given? Is not the water always cool and sparkling, the cesspool out of sight, the graveyard old and long disused? In one case at least these conditions are not all fulfilled, as the following extract will show—sufficiently startling in its simple horror, and needing no comment or explanation :—"I stood by *the* well at Cawnpore, and I thought how dreadful it was to realise the fact that all the other wells in the surrounding cantonment are in communication with the common grave of our murdered fellow-countrymen and women, 'whose bones lie scattered in that pit,' as the inscription upon their monument declares."*

* A. S. Ormsby. "A New Idea for the Water Supply of Towns, and also for the Troops in India and the Colonies," p. 16. 1867.

THE CRETACEOUS FLORA.

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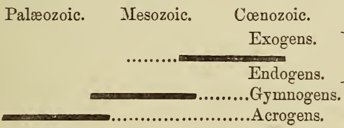
[PLATE CXXX.]

THE conditions under which the various sedimentary strata were accumulated, and the inferences deducible as to the physical geography and climatal characters of the different periods, may be understood not only from the study of the sediments themselves, but more especially from the nature and relations of the remains of the fossil animals and plants imbedded in them. The greater portion of the vast thickness of the stratified formations (estimated at from twelve to sixteen miles, and even more, in thickness) are chiefly due to marine agency, with some intercalated beds of estuarine and fluviatile origin, while others are indicative of old terrestrial surfaces which must have been to some extent more or less contemporaneous with the aqueous deposits then in progress. The evidence of this old land is inferred from the nature of the vegetation and the occurrence of the remains of air-breathing animals associated with it. The general geological belief appears to be—from the nature of the evidence preserved to us in the successively formed fossiliferous rocks—that from the earliest period to the present there has been a gradual increase of the land-surface of the globe. The remains of the animals and plants all point in this direction.

In the Upper Palæozoic period there was a luxuriant land vegetation, some insects, and air-breathing reptiles; in the Mesozoic period many land plants, insects, air-breathing reptiles and warm-blooded mammals, chiefly marsupials, and in the succeeding Tertiary or Cœnozoic period a considerable increase in numbers of the orders and families of animals and plants, both of which have attained their maximum at the present time.

With regard to the development of the vegetable kingdom in time, Mons. Adolphe Brongniart many years since divided it into three great periods corresponding to those above mentioned:

1, the reign of Acrogens, characterised by the abundance of *Equisetaceæ* (horsetails), *Lycopodiaceæ* (clubmosses), *Filices* (ferns); 2, the reign of Gymnogens, characterised by the cycads and conifers; 3, the reign of Angiosperms, characterised by the exogens and endogens. Not that they are special or peculiar to each period, but are the most abundant forms; so that, instead of being a truly consecutive series, they form a more or less parallel one; for in the first period the abundant acrogens are associated with some gymnogens in the form of the coniferæ, in the second period the numerous gymnogens are associated with some angiosperms, which latter increase and constitute the dominant flora of the third period, as shown in the following diagram:—



The earliest well-marked flora is that of the Devonian period, as seen in New Brunswick, Ireland, and some other parts of Europe. In the underlying Silurian (although traces of *Lycopods* are stated to occur) and in still older strata, the *structural* evidence of plants is very scanty; still the occurrence of anthracite, veins of graphite, and beds of iron ore, indicate probably the existence of a former vegetation, whose original characters are so obliterated as to prevent us from determining their real affinity.*

The Mesozoic rocks, characterised by their gymnospermous flora, are divided in ascending order into Triassic, Jurassic, Purbeck-Wealden, and Cretaceous strata, and it may be interesting to inquire what was the nature of the plant-life at the close of the mesozoic period, and to what extent it was related to a preceding, or foreshadowed that of a subsequent period. Comparatively recent discoveries in Europe and America have considerably enlarged our knowledge of this old cretaceous vegetation, from which it appears that, with some forms in common, it is markedly distinct from the lower mesozoic flora, and presents the facies of a tertiary flora, especially in the abundance of dicotyledonous plants, so much so that in some districts the beds containing them were at first considered to be of miocene instead of cretaceous age.

Essentially a marine formation, and consisting of sediments

* Sterry Hunt, "Amer. Jour. of Science," 2nd ser. vol. xxxi. p. 395. C. H. Hitchcock, "Geology of New Hampshire," pp. 508, 509.

of clay, sandstone, and limestone either of mechanical, chemical or organic origin, the Cretaceous rocks are widely spread over the globe, occurring in Europe, Greenland, North and South America, New Zealand, India, and the Pacific Islands. According to locality these rocks vary in mineral aspect, and indicate either deep or shallow water deposits, proximate lines of shore from which drift wood was carried into deeper water, or land-surfaces upon which grew for a long period a sufficiently luxuriant vegetation to form moderate beds of coal, as in New Zealand, Vancouver's Island, and other places. It is the nature of this vegetation we shall attempt to explain, as derived from the various localities where plant remains have been preserved.

In the British area and adjacent parts of France the cretaceous plants are very few. The Lower Greensand or upper Neocomian shows, however, that the land of the Wealden period was not entirely submerged, for with its marine shells are found the Wealden fern (*Lonchopteris Mantelli*), the Wealden Iguanodon, the cycadeous genera *Yatesia* and *Mantellia*, and some cones and stems of coniferæ.

In the overlying gault at Folkestone coniferous fruits have been found, referred to the genus Wellingtonia (*Sequoia*), and others more nearly related to that group of the section *Pinea*, the members of which are now associated with the Wellingtonias in the west of North America, than to any other members of the great genus *Pinus*, which seems to point to the existence of a coniferous vegetation on the high lands of the upper cretaceous period, having a facies similar to that now existing in the mountains on the west of North America, between the thirtieth and fortieth parallels of latitude. No fossil referable to *Sequoia* has hitherto been found in strata older than the gault, and here on the first appearance of the genus we find it associated with pines of the same group, species of which now flourish by its side in the New World.*

In the gault, upper green sand, and white chalk, besides coniferous fruits, fragments of coniferous wood are occasionally found, the latter sometimes having perforations made by a species of *Teredina*.

On the continent of Europe, however, rocks of Cretaceous age have in sundry localities yielded a more or less abundant land flora, distributed at different geological horizons from the lower Neocomian to the Danian or uppermost chalk.

To the Urgonian or middle Neocomian belong the bituminous schists and sphærosiderites of Teschen and Wernsdorf † in the

* Carruthers, "Geol. Mag." vol. viii. p. 540. A. Gray, "Address," 1872, p. 5.

† Schenk, "Die fossilen Pflanzen der Wernsdorfer Schichten in den Nordkarpathen." Palæontographica. Bd. xix.

northern Carpathians, containing numerous vegetable remains. The plastic clays of La Louvière, in Hainaut (Belgium), referred to the gault, contain almost exclusively remains of coniferæ and cycadeæ, and thus their flora differs essentially from that of Aix-la-Chapelle, although it is not far distant, for the latter comprises many dicotyledons but no cycad, and the species and even the genera of coniferæ are different. According to M. Coemans, of all the cretaceous floras known at present, that of La Louvière presents the remarkable peculiarity of not containing any species common to the other floras of the same period. England, Saxony, Silesia, and Moravia, have yielded Cretaceous coniferæ, but they have little or no relation with the Belgian species.*

It is one example of an isolated flora of the period. Besides which the flora of Hainaut seems to afford a series of intermediate types with existing genera: thus *Pinus Corneti* (Coem.) connects *Abies* with *Cedrus; P. Andräi*, *Strobus* with *Pinaster*; and *P. Heeri* forms a transition from *Cembra* to *Strobus.*

The upper greensand and lower chalk corresponding to the Cenomanian and Turonian of France, and the lower quadersandstein of Germany and Austria, contain many gymnosperms (coniferæ) and dicotyledons, the latter group here for the first time appearing in any abundance; for the geological position of the Aix-la-Chapelle beds (hereafter noticed) is newer. Strata containing plants referred to the former age occur at Neustadt, in Austria; Niederschöna and Goppeln, in Saxony; Hradek, Kutschlin, Laun, Perutz, Smolnitz, Trziblitz, in Bohemia; Oppeln and Tienfenfurth, in Silesia; and Moletein, in Moravia. Plant-bearing beds of the upper chalk or senonian and upper quadersandstein have been noticed at Blankenburg and Quedlinberg, in the Hartz; at Halden and Sonderhorst, in Westphalia; and it is probable that the Platten-kalk of Westphalia and the sandstone of Klin, near Moscow, containing *Sequoia* and other conifers, belong to this age.

The most interesting locality for fossil Cretaceous plants at present known in Europe is undoubtedly that of Aix-la-Chapelle, where Dr. Debey has noticed more than 400 species, generally well preserved, belonging to all the great divisions of the vegetable kingdom, of which the *Dicotyledonous angiosperms* are the most prominent feature of this old flora.

The Aachenian sands of Aix-la-Chapelle belong, according to MM. Bosquet, Schimper, and Sir C. Lyell, to the upper chalk or senonian.†

* "Mémoires de l'Académie royale de Belgique," tome xxxvi.

† Bosquet, "Foss. Fauna en Flora, Starings Bodem van Nederland," IIde Deel. Schimper, "Palæontologie Végétale," tome iii. p. 673. Lyell, "Elements of Geology," 1874, p. 286.

The fossil plants and associated seams of lignite, and of more or less perfect coal, are intercalated with beds of sand and clay of considerable thickness, evidently derived from some neighbouring land, and interstratified with them are occasional bands of limestone with marine shells. "On the whole," says Sir C. Lyell, "the organic remains and the geological position of the strata prove distinctly that in the neighbourhood of Aix-la-Chapelle a gulf of the ancient Cretaceous sea was bounded by land composed of Devonian and carboniferous rocks. These rocks consisted of quartzose and schistose beds, the first of which supplied white sand, and the other argillaceous mud, to a river which entered the sea at this point, carrying down in its turbid waters much drift-wood and the leaves of plants. Occasionally, when the force of the river abated, marine shells of the genera *Trigonia*, *Turritella*, *Pecten*, *Hamites*, &c., established themselves in the same area, and plants allied to *Zostera* and *Fucus* grew on the bottom." *

But the position of the Aachenian sands in Belgium and the north of France, which are at the base of the Cenomanian, has been differently assigned, and were the subject of considerable discussion at the annual meeting of the Geological Society of France at Avesnes (1874).† M. Gosselet refers them to the gault. M. de Lapparent, agreeing with the late M. Dumont, believes they correspond to the Wealden; and MM. Cornet and Briart consider their formation to have commenced at the close of the carboniferous period, and to have continued, with all probability, until the termination of the gault.‡

The flora of Aix-la-Chapelle was essentially terrestrial. A few algæ occur, marine contemporaries of the land vegetation, which comprised numerous ferns, of which three of the genera are at present living: Asplenium, now European, and widely distributed; Lygodium and Gleichenia, not European, the former mostly tropical, and the latter chiefly found in the South of Africa and Australia. The coniferæ were tolerably abundant, including Glyptostrobus, Sequoia, and Araucaria, associated with some new types, as *Cycadopsis*, near to Sequoia, and Moriconia related to the Cupressineæ in resembling *Libocedrus* and *Thuiopsis* of the present flora. Among the Monocotyledons, species of *Pandanus* or screw-pine and other forms occur. Of the dicotyledonous angiosperms, which constitute more than half of the flora, the Proteaceæ—a family now restricted to South Africa, the Cape, and Australia—are very

* "Elements of Geology," 1874, p. 286.

† "Bulletin Soc. Géol. de France," 1875.

‡ "L'Aachenien et la limite entre le Jurassique et le Crétacé dans l'Aisne et les Ardennes," par M. Ch. Barrois. Bull. Géol. Soc. de France, 3 ser., tome iii. p. 257.

predominant. Besides extinct genera there are others, considered to belong to living forms, as Adenanthus, Banksia, Dryandra, Grevillea, Hakea, and Persoonia. Associated with these Proteaceæ are the fig, bog-myrtle, willow, poplar, the oak, beech and dryophyllum, a cupuliferous genus allied to the chestnut-oak, also Credneria, and cissites related to the Ampelideæ, together with some forms of the Myrtaceæ, as *Eucalyptus*, &c.*

The eastern continent, like the western, affords equally interesting facts as regards the old land surfaces of the Cretaceous period. In Spitzbergen and Greenland, under latitude 70° N.—where now almost perpetual ice and snow reign, with the disappearance of the snow there suddenly springs forth a herbaceous vegetation, rushing into leaf and flower, to be speedily destroyed by the return of the winter's frost after a very short summer—there is evidence of a forest vegetation at the Cretaceous and Miocene periods, indicative of a temperate or even subtropical climate, thus showing that great physical and cosmical changes have taken place in the Arctic regions, and inferring at those periods a different distribution of land and of warm oceanic currents from that which obtains at present. The researches of Professor Nordenskiöld, in connection with the late Swedish expeditions, and the subsequent determinations of Professor Heer, have thrown considerable light on the ancient floras and climates of the Arctic regions.† The Cretaceous strata of Greenland, according to Professor Heer, belong to two different stages, and contain different floras. The lowest division is met with at Kome or Kook, and other places on the north side of the Noursoak Peninsula in North-western Greenland. These strata, considered to be of middle Neocomian age (Urgonian), contain some plants which resemble those of Wernsdorf in the Carpathians. About seventy-five species are described, belonging to fifteen families; more than one-half are Cryptogams, chiefly of the fern tribe, among which the species of Gleichenia play the chief part, a genus which still flourishes in the vicinity of the tropics and warmer portions of the temperate zone; but *Sphenopteris* and *Pecopteris* are not rare. The other plants are chiefly gymnosperms—the Coniferæ—some of which are nearly related to forms still existing in Florida, Japan,

* Debey, H. M. "Uebersicht der urweltlichen Pflanzen des Kreidegebirges, überhaupt und der Aachener Kreideschichten insbesondere."—Rhein1. u. Westphal. Verhand. 1848, pp. 113–142.

Debey, H. M. und Ettingshausen. "Die urweltlichen Thallophyten des Kreidegebirges von Aachen und Maestricht."—Wien. Denkschr. Akad. Wiss, xvi. 1859, pp. 131–214. "Die urweltlichen Acrobryen."—Ibid. xvii. 1859, pp. 183–248.

† "Flora fossilis Arctica."—Zurich, 1868, 1875.

and California (*Sequoia*), and Cycadeæ, most of which are referable to the genus *Zamia*, species of which are now met with within the tropics. A leaf was also found. From the general nature of the flora Heer considers that, in the early part of the Cretaceous period, the climate of the now ice-covered Greenland was somewhat like that which now prevails in Egypt and the Canary Islands.

The upper Cretaceous beds, referred to the Cenomanian or Turonian, occur at Atane and on the shore below Atanekerdluk, on the south side of the Noursoak Peninsula. The number of species is nearly equal to that found in the lower Cretaceous at Kome, but their type is almost totally different, although five species are common, but they are quite unlike the Greenland Miocene plants. *Sequoia*, again, predominates among the conifers, and with it a *Thuites* and *Salisburea* were found; cycads are rare. Among the ferns there are still some *Gleicheniæ*, but other forms, as *Marattiaceæ* and *Dictyophyllum*, have disappeared. The predominant forms are dicotyledons; there are three species of poplar, one fig, one *Myrica*, a *Sassafras*, a *Credneria*, some *Proteoides* and *Leguminosites*, and two *Magnoliæ*.*

Professor Nordenskjöld, in speaking of this Cretaceous flora, in a lecture to the Royal Swedish Academy (1875), says, "Among the ferns, cycadeæ, and coniferæ of Noursoak Peninsula (lower Cretaceous) were found a few impressions of a species of poplar, *Populus primæva*, which formed the only and at the same time the oldest known representative of the forest vegetation now prevailing in the temperate zone. Nevertheless, the vegetation of the Arctic tracts was already during the Cretaceous period undergoing a complete transformation. Evidence of this has been obtained from the same locality, Atanekerdluk, on the south side of the Noursoak Peninsula. Here impressions of plants may be discovered belonging to the Cretaceous formation, not to the lower but the upper portion of it. The vegetation is here quite different. The ferns and cycadeæ have disappeared, and in their place we find deciduous trees and other dicotyledons in astonishing variety of forms, among which a species of fig may be mentioned, of which not only the leaves but also the fruit have been obtained in a fossil state; and species of Magnolia, Sassafras, Proteoides, &c. The climate that then prevailed over the whole globe was therefore still warm and luxuriant, even if, at least in the Arctic regions, considerably modified from what it formerly had been, inasmuch as the flowerless vegetation (which was now beginning to die out)—as far as we can judge from its present representatives, the ferns—required a warm humid climate;

* See P. H. Scott, "Geol. Mag." Feb. 1872, p. 71.

whereas the new forms, with their luxuriant flowers, which now began to characterise the vegetable world, required, in order to develope all the grandeur of their colours, a clear and sunny sky. The disappearance of sundry tropical and sub-tropical forms, that are met with in the older Cretaceous strata, has led Heer to the conclusion that difference of climate at different latitudes was now beginning to show itself; and he calls attention to the circumstance that this takes place synchronously with the development of the dicotyledonous plants in greater variety." *

The Cretaceous character of the strata containing coal and lignite at Vancouver's Island and adjacent district was some years since noticed by Dr. Newberry, and these and their extensive development along the Pacific coast have been described by Dr. Hector.

At Nanaino—where the coal has been largely worked, as on Newcastle Island—there are two seams, the first of which is about six feet in thickness, with sometimes a floor of fire-clay, but more generally of sandstone; and the roof, consisting of the fine conglomerate bed, about sixty feet thick, on which rests the Douglas seam, with an average thickness of about four feet.†

The coals and fossil plants of Vancouver's Island were considered at one time to be of Tertiary age, but this identification appears to have been made upon insufficient evidence. Among the fossil plants there is one, the *Sequoia Reichenbachi*, which is numerous in localities in the Cretaceous of Europe and in the Arctic regions. Interstratified with and overlying the beds of coal at Nanaino are strata containing great numbers of well-marked Cretaceous mollusks—*Ammonites*, *Baculites*, *Inoceramus*, &c.‡

The most abundant North American Cretaceous flora is that of the Western Territories, collected from beds known as the Dakota group, by the labours of Dr. Newberry, and the geological survey under Dr. Hayden, and recently described by Professor Lesquereux.§

The Dakota group lies at the base of the Cretaceous series, and, with the associated Fort Benton group, is equivalent to the Cenomanian and Turonian of Europe; overlying these are many hundred feet of strata, destitute of plants, referred to the upper chalk or Senonian, and above these are the remarkable lignitic beds or "Transition Series" of Dr. Hayden, which

* "Geol. Mag." Nov. 1875, p. 529.

† Hector, J. "Quart. Journ. Geol. Soc." vol. xvii. p. 433. See also Newberry, "Proceed. Boston Nat. Hist. Soc." 1863.

‡ J. S. Newberry, "American Journ. Science and Arts," April 1874.

§ "The Cretaceous Flora." Washington, 1874.

from their fauna are considered by Dr. Cope to bridge over what is generally considered one of the greatest gaps in geological chronology. The Dakota group, corresponding to the Upper Cretaceous of Europe, like the beds of Aix-la-Chapelle and the Aachenian sands of Belgium and others of Bohemia, are immediately superposed on the Palæozoic rocks, thus showing a great physical break, or lapse of unrepresented time, during which in other areas thick marine sediments were accumulating.

The Dakota group has been recognised to have a vast range along the flanks of the various mountain-ranges from north to south, extending from North Texas to the northern limits of the State of Minnesota, is presumed to extend into British America, and as the fossil leaves of the Upper Cretaceous of Greenland represent some genera and perhaps species of the Dakota group, it would seem that this formation has been continuous from the Gulf of Mexico to the Arctic lands, Greenland, &c., over 35° of latitude. The fossils have been chiefly found on the plains in the eastern portions of Kansas and Nebraska.

About 130 species of plants have been determined, including six ferns, nine coniferæ (Sequoia, &c.), one cycad, three monocotyledons; the remainder are dicotyledons, and represent species referable to the genera *Liquidambar*, *Populus*, *Salix*, *Betula*, *Myrica*, *Celtis*, *Quercus*, *Ficus*, *Platanus*, *Laurus*, *Sassafras*, *Cinnamomum*, of the Apetaleæ; *Diospiros* of the Gamopetaleæ; *Arolia*, *Magnolia*, *Liriodendron*, *Negundo* or *Acer*, *Paliurus*, *Rhus* or *Juglans*. and *Prunus* of the Polypetaleæ; or, merely considering the affinities to our present flora, seventeen genera are those to which belong the species of our trees and shrubs which have the more general and the widest range of distribution. Indeed, all our essential or arborescent types are there, except those which are marked by serrate or doubly serrate leaves. This general facies of the leaves of the Dakota group, viz. integrity of borders and coriaceous consistence of leaves, is peculiar.

The borders, if not perfectly entire, are merely undulate or obtusely lobed; to this there is scarcely an exception. This mode of division of the borders of leaves is very rare in species of our present times, except perhaps in some species of poplars.*

Considered as a whole, most of the types of the Dakota group, related to those of our present flora, represent a moderate climate, like the one prevailing now between 30° and 45° of North latitude. Professor Heer has the same opinion in regard to the climate of the upper Cretaceous epoch of Greenland, as indicated by its flora.†

* See Lesquereux, "The Cretaceous Flora," p. 128. † Ibid. p. 39.

With regard to the conditions of deposition, Professor Lesquereux remarks, that the character of the leaves found in the Dakota group, and their analogy with species of our time, seem at first to refer them to a dry land flora; this is, however, not positively the case. The most abundant representative of the Cretaceous flora, the Sassafras, is remarkably similar to the present *Sassafras officinalis*, which inhabits every kind of ground and station, from the dry hills of Ohio to the low swamps of Arkansas. The numerous leaves of Laurus are comparable to those of *L. caroliniana*, a shore plant; as are also the *Platanus*, *Magnolia*, *Populus*, *Salix*, *Menispermites*; the essential types of the Dakota group being, therefore, those of low islands or low shores, rather than hills or dry land. Professor Mudge says the characteristic of the local deposits of this group in the State of Kansas indicate that the forests were on small islands scattered over the Cretaceous ocean.*

In Cumberland and Nelson provinces, New Zealand, rocks referable to the Cretaceous age by their fossils overlie valuable coal-seams, locally altered into anthracite, and associated with the shales and sandstones is a rich dicotyledonous flora, both angiosperms and gymnosperms, as the Myrtaceæ, Fagus, Coprosma, Dammara, Phyllocladus, Podocarpus, Dacrydium, &c., many of the genera still existing in the New Zealand flora. There is one locality at Pakawu, where the genera Pecopteris and Tæniopteris are clearly associated with the dicotyledonous plants, the species however being different from those representing the same genera in an underlying sandstone, which is undoubtedly of Jurassic age, and which species are mostly identical with those found in the Rahmajal series of India.

Overlying the coal-bearing rocks are a series of conglomerates, sandstones, calcareous marls and chalk, with flints in some localities more or less developed, and there is evidence in the upper part of this series of fossils with an Eocene facies, succeeded by lignite beds indicating a land surface.†

The Cretaceous period may be divided into two distinct periods according to the nature of its terrestrial vegetation, and these somewhat agree with the ordinary geological divisions; including the Wealden, Neocomian, and Gault in the lower; and upper green sand, lower, middle, and upper chalk in the upper division.

M. Schimper has divided the Cretaceous flora as follows:—Etage Inférieur (Neocomien, Urgonien), Etage Moyen et Supérieur (Aptien to Danien).

The general Cretaceous flora includes: Thallogens, as Algæ; Acrogens, as ferns and equisetum; Gymnogens, as conifers

* "Trans. Kansas State Board of Agriculture," p. ?95.

† Dr. Hector, "Trans. New Zealand Institute," vols. ii. and vi.

and cycads, a few monocotyledons, as palms; dicotyledons, as the ordinary forest trees; but it is the different distribution of these classes that forms the marked feature of the two divisions. In the lower group, the gymnogens and cryptogams are abundant; in the upper, the dicotyledons are most numerous, with some conifers and ferns. This distribution has some relation to the floras preceding and succeeding the Cretaceous period. Thus the preceding Jurassic flora is composed of *ferns*, a few Equisetaceæ, some Coniferæ, and a great abundance of Cycadeæ. Three-fourths of all the fossil *Zamiæ* and half of the Cycadeæ, known from all the geological formations, are Jurassic. In the lower Cretaceous of Wernsdorf and Greenland, especially the latter, Heer finds a marked proportion of this family which is scarcely represented in the upper. Of the *Coniferæ* the lower Cretaceous rocks (Gault) contain the earliest known representative of *Sequoia*; this genus is interesting, not only as a persistent type ranging from the Cretaceous to the present, but from its wide distribution at former geological periods, *i.e.* the upper Cretaceous and Miocene, and being now represented by two species only (the Redwoods), and those restricted to a comparative limited area in North America, the *S. gigantea* (Mammoth tree or Wellingtonia) to the western flank of the Sierra Nevada, and *S. sempervirens* along the coast range from the bay of Monterey to the frontiers of Oregon.* The absence of dicotyledons, the presence of a few monocotyledons, and the abundance of the Cycadeæ. approximate the lower Cretaceous to the previous Jurassic flora.

On the other hand, the Upper Cretaceous presents a distinct assemblage, and affords the first well-marked proof of the introduction on the earth of a vegetation allied to our fruit and forest trees.† Among the ferns are the living genera Gleichenia, Lygodium, Asplenium, together with the earlier forms Sphenopteris and Pecopteris; scarcely any Zamiæ, but some Coniferæ, as Abietites, Phyllocladus (?) Sequoia, and its near relative Glyptostrobus, now living in China. Of the numerous genera of dicotyledons are the fig, willow, poplar, beech, oak, plane, sweet-gum-tree (Liquidambar), sassafras (abundant in the Dakota group), Magnolia, Liriodendron (tulip-tree), some Proteaceæ, and other living and extinct genera, including *Credneria*, many of them having at present a very different geographical distribution. Among others, the *Magnolia* and *Liriodendron* belong to North America by origin, succession, and presence. Of the eight species

* See Lesquereux, "Cretaceous Flora," p. 116; and Professor A. Gray, "Address to the American Association at Dubueque, Iowa," Aug. 1872.

† Lesquereux remarks that "the essential types of our actual flora are marked in the Cretaceous period, and have come to us after passing, without notable changes, through the Tertiary formations of our continent."

of true Magnolia now known to botanists, seven belong to the western slope of the temperate zone of North America. *Liriodendron*, the Tulip-tree, has in its characters, its distribution, and its life, a great degree of affinity with *Magnolia*; the American species is the only one now known in the vegetable world, and its habitat is strictly limited to America. Either considered in its whole or its separate characters, the tulip-tree is a constant subject of admiration and wonder. It could be named, not the king—it is not strong enough for that—but the queen of our forests, if the *Magnolia* was not there with it to dispute the prize of perfection by the still grander majesty of its stature, the larger size of its foliage, the elegance and perfume of its flowers. Our sense of admiration for these noble trees is heightened still by the dignity of their ancient origin.*

The upper Cretaceous rocks, therefore, by their numerous forms of dicotyledons strongly foreshadow the Tertiary and succeeding floras. As partly bearing on this point, Professor Lesquereux states, as regards the Western Territories, "In ascending from the lower lignitic measures, where the essential types of the Cretaceous flora (the Dakota group) have no representatives, we see these Cretaceous types re-appearing, a few in the Upper Eocene, more in the Carbon group above, still more in the Upper Tertiary, following thus an increasing degree of predominance, culminating, it seems, at the present time, in the flora of the eastern slope of the North American continent."†

A character of the Cretaceous floras is their apparent want of homogeneity; even when probably synchronous, they are so diversified, when compared to each other, as to appear not to belong to the same epoch and the same country. On this point MM. Saporta and Marion remark, in alluding to the localities which have been carefully studied, "What point of analytical connection can be established between Niederschœna in Saxony, Moletin in Moravia, Quedlinburg and Blankenburg in the Hartz, Halden in Westphalia, the sands of Aix, the Senonian of Bausset, the Santonien of Fuveau, and the North American Cretaceous of Nebraska?"‡ Professor Lesquereux also shows that the Dakota flora, with scarcely any forms referable to species known from coeval formations of Europe, presents in its whole a remarkable and as yet unexplained case of isolation.

Contemporary, therefore, with the extensive marine deposits of this period—some of a peculiar nature, great thickness, and uniform character, as the white chalk, indicating deep-sea conditions—there were evidently land surfaces either distributed as islands in the cretaceous ocean, or forming more or less low

* Lesquereux, "Cretaceous Flora," p. 121. † Ibid. p. 117.

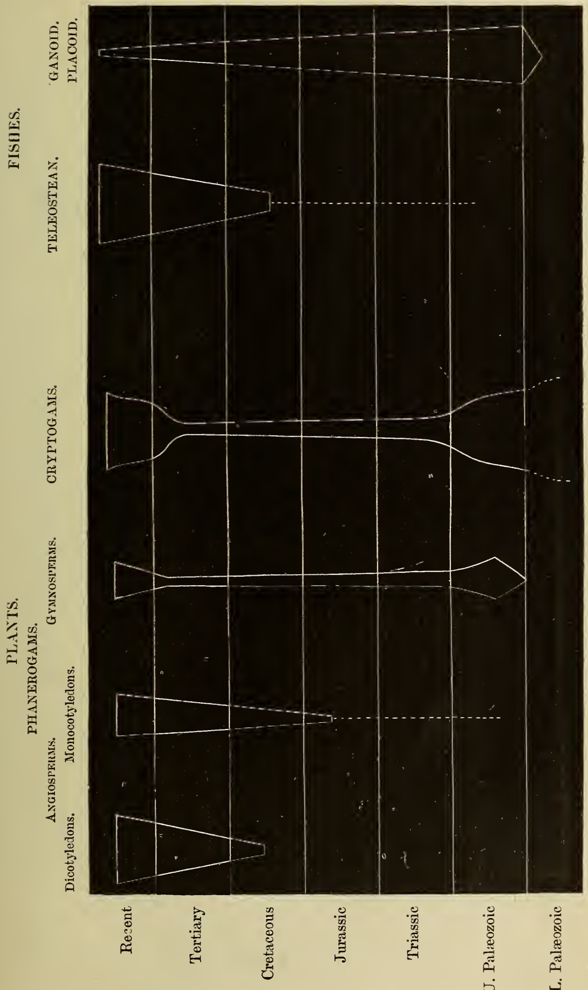
‡ "Essai sur l'état de la végétation à l'époque de marnes heersiennés de Gelinden." Par Count Saporta et Dr. F. A. Marion. P. 14.

shores bordering the same, upon which grew a vegetation varying in character, and sometimes localized, according to climatal conditions; in some places sufficiently luxuriant to become the source of future beds of coal; in other places, to be carried by rivers or other means and imbedded with the marine remains of the adjacent sea. This vegetation was not, however, of uniform character throughout the Cretaceous period, but became modified and varied in the various stages, and with a marked difference in the lower and upper periods. In the lower, with the incoming of a new marine fauna, the plant-life resembled that of the previous Jurassic age; whilst in the upper, with a comparative similar aspectal fauna, the cotemporary vegetation was different from that of the lower Cretaceous period, and presented a land facies resembling that characteristic of the subsequent Tertiary period, in which the forms of the cotemporary marine life were entirely changed.

In recapitulating the evidence derived from our present knowledge of the distribution and character of the Cretaceous flora, it appears—1. That in the different localities from which the plants have been described, while the floras have some points in common, they are more or less localised in character, probably in consequence of different physical and climatal conditions. 2. That the Upper Cretaceous flora differs considerably from the Lower, inasmuch as the latter, by the predominance of the gymnosperms, is related to and continuous with the previous Wealden and Jurassic flora; while the former, containing a few of the older forms, is marked by the abundance of the dicotyledons, thus foreshadowing the subsequent Tertiary flora. 3. That while in some areas the Cretaceous flora yields generic forms, which are now represented and living in the same area—as for example in New Zealand, Europe, and America—in other places, as in Greenland, there are no such living representatives; and again, some genera, as *Sequoia*, which were widely distributed during the Cretaceous period, are now restricted to a single locality. 4. That the appearance of a phænogamous flora in the upper Cretaceous rocks affords a parallel to the occurrence in the same formation of the teleostian fishes, both of which classes not only increase in the subsequent Tertiary period, but are the dominant forms at the present day, as shown in the accompanying Pl. CXXX.

These occurrences may indicate either distinct creations, adapted to the then existing inorganic conditions, or, when our knowledge of the earlier forms is more extended, it may be shown to have resulted by modification during long periods of time of previous generalised or ancestral types, of which there is some evidence in the underlying Jurassic and still older formations.

PLATE CXXX.



Range of Plants and Fishes in Time.

THE "INFLEXIBLE" AND HER ARMAMENT.

By A. HILLIARD ATTERIDGE.

[PLATE CXXXI.]

THE old type of man-of-war is now fast becoming a thing of the past. The splendid three-deckers and swift-sailing frigates that carried our flag to victory in the days of Jarvis and Nelson, or that only twenty years ago engaged the sea-forts of Sebastopol and blockaded the Baltic shores of Russia, will soon be as obsolete as the triremes of ancient Rome and the "tall galleons" of the times of the Armada. Already they have disappeared from the line of battle, and are relegated to the humble position of guardships at our home ports or cruisers on distant stations, where their most serious enterprises are the bombardment of a village or the capture of a slaver. One by one they are condemned and broken up, and no new ships are laid down to replace them; and already we can anticipate the time when perhaps the sole representative of the grand old floating fortresses, that once formed our great unarmoured fleet, will be Nelson's ship, the *Victory*, lying at her moorings in Portsmouth harbour like some war-worn veteran of Greenwich or Chelsea whose fighting days are over, and who is spending his old age in honourable retirement.

The modern man of war is much more than an armed steamer. She is herself a great engine of destruction, provided with huge guns, clad in heavy armour, driven by powerful engines, and able to send an adversary to the bottom by one successful blow of her enormous bow. Year by year the thickness of armour and the weight of naval artillery go on increasing together: mechanical appliances have more and more replaced manual labour both in the dockyard and on shipboard; and at the same time the form of the ships themselves has been carefully adapted to the work they have to do and the conditions under which they must act. Our first great ironclad, the *Warrior*, was only an ordinary war-steamer very incompletely protected with armour,

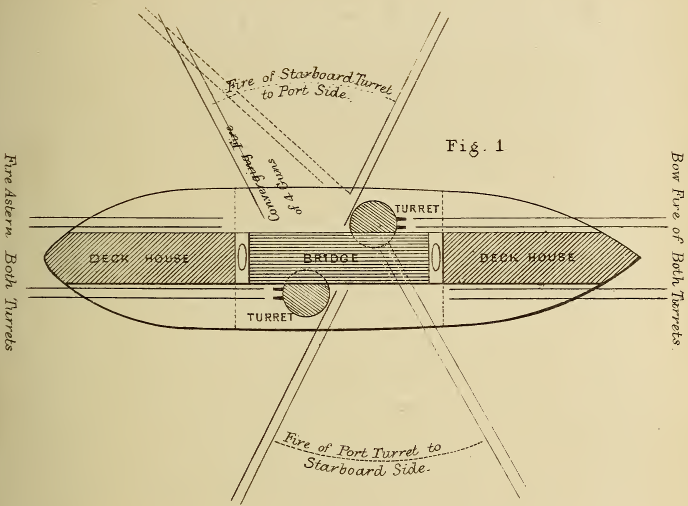


Fig. 1

Fig. 2.

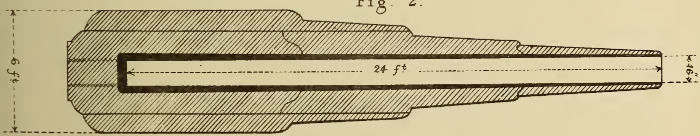
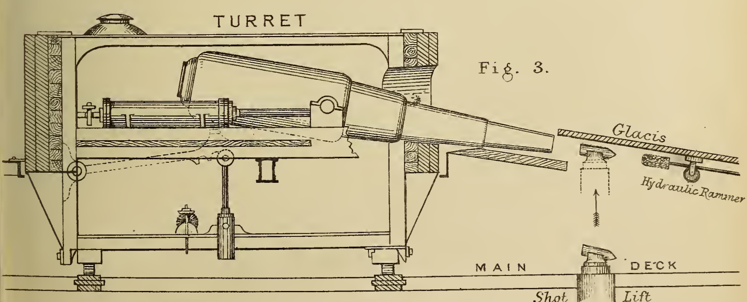


Fig. 3.



A.H.A. del.

W West & Co imp

The Inflexible and her Armament.

but quite sufficiently to resist the guns afloat in foreign navies at the time. Her armour was only $4\frac{1}{2}$ inches thick, her heaviest guns were 68-pounders, weighing 95 cwt. Her immense length of 380 feet was exceeded by that of the *Minotaur* and her sister ship the *Northumberland*; but it was found that these long ships were not well adapted for manœuvring in line of battle, and later ironclads were made gradually broader in the beam, and shorter in the length from stem to stern. At the same time various minor improvements were introduced into the build, the most important of which was the change of the old oblique projecting bow into the almost perpendicular "swan-breasted" shape, which is substantially the same as that of the present running-down bow or ram. The armour was no longer restricted to the midship portion of our war ships; it was extended fore and aft, until they were completely covered above water and a few feet below it. The weight of the guns steadily increased, and with it the thickness of armour, while turrets and the tripod system of rigging were employed to give a concentration of fire on any desired point. The *Bellerophon*, with 12-ton guns, was given 6-inch armour; the *Hercules*, with 18-ton guns, armour of 9 inches; the *Devastation* carries 35-ton guns and armour of 12 inches on her sides, and 14 on her turrets; and the *Inflexible*, now building at Portsmouth, will have armour two feet thick and four 81-ton guns.

This turret ship is remarkable as the highest development of the modern fighting ship—for that is the best way to describe her. The navies of Europe are fast being divided into ships for coast defence, for cruising, and for action in line of battle in great naval engagements; and while fully available for the first of these purposes, the real object of the *Inflexible* is the last. There cannot be a doubt that she will be the most powerful man-of-war ever launched, though he would be a rash prophet that would predict that she will not ere long be left behind in the race of improvement by some still more formidable turret ship.

The *Inflexible* will be 320 feet long on the water-line, and will have a breadth of beam of 75 feet. The hull will consist of two parts—the main substructure and the upper portion; the former being an iron hull, no part of which will be less than six or seven feet under water. It will be built with a ram-bow and provided at the stern with a rudder and a pair of twin screws. On this is erected the armoured central or fighting portion of the ship, which will have a height of 10 feet above the water-line, and will be 110 feet long. Upon its deck will be the two turrets, each armed with a pair of 81-ton guns. At both ends of this midship section rises a lighter structure of the same height, but having along its centre, running fore and aft, deck-houses 10 feet long and 30 feet wide (Fig. 1, Pl. CXXXI.). The deck-houses being prolonged to the bow and stern, will give a poop and a forecastle for working the anchors twenty feet above the water. A broad bridge passing over the turret-deck will connect them, and thus give an even upper deck 30 feet wide and more than 300 feet long, extending from stem to stern. The position of the turrets in the *Inflexible* has been made the subject of a novel arrangement. They are placed at each end of the central deck—not in an even line with each other, but diagonally at opposite corners of it, so that one turret is on the starboard and the other on the port side. The effect of this arrangement is that all the four guns have an uninterrupted range of fire all round the horizon. In firing ahead or astern the guns are trained so as to send their shot over the level portions of the deck on either side of the deck-houses (Fig. 1). In firing to starboard the port turret unites its fire with that of the starboard turret by aiming under the bridge, and *vice versâ*. Thus while in all our other double-turreted ships there is a fire of four guns on either beam, but of only two guns ahead or astern, the *Inflexible* will be able to direct her four guns at an object in any direction with respect to herself. The ship will have two or three masts, jury-rigged; none of the stays or running rigging will be brought down to the lower deck so as to interrupt the fire of the guns, all the working of the ship being carried on on the upper platform. Thus, by a simple and novel arrangement, the turret-system has been brought to what we may call perfection.

The *Inflexible* will have four sets of engines, with an aggregate of 7,000 horse power. Her full speed, with both screws going, will be 14 knots an hour; but on ordinary occasions she will be able to economise fuel by working only one screw and its engines. At the speed of 10 knots an hour she will be able to carry coals for a cruise of 3,000 knots, or twelve and a half days, which is about the average coal-carrying power of the best ships of our ironclad fleet. She will also be able to use some auxiliary sail-power; and, independent of this, her try-sails will be valuable in steadying her, and keeping her head to the wind in heavy weather.

Only the central portion of the ship and the two turrets will be armoured, the former with two feet, the latter with a foot and a half of armour, for even if the lightly built ends were riddled with shot, the ship would still keep afloat. In these ends are the coal-bunkers; when full it would make very little difference even if water got in among the coal, and when they are empty the ship would be much lighter, and have more floating power. But whether empty or not, the ends will not be wholly unprotected. A narrow passage will lead round them at the water-line, just inside the inner skin of the ship. The sides of this passage will be lined with cork, so that a shot passing through it will make a small clean hole. At various points in the passage masses of packing will be ready to be shoved backwards or forwards to the place where the shot has passed through, so as to block up the passage there, and stop the hole until it can be propeily plugged. The armour of the central portion will be the heaviest ever yet floated. It will be no less than two feet thick; but this does not by any means express its full resisting power, for it is arranged on a new system which will materially increase its strength. It will be bolted on in two layers, each 12 inches thick, but between these there will be a 9-inch layer of solid teak, and behind the whole a heavy teak backing, and then the iron framework of the ship's side. Now a shot from a very powerful gun, on striking this bulwark of wood and iron, would probably penetrate the outer plate, but in doing so it would be broken in several fragments; and these, after tearing their way through the layer of teak, would encounter the inner plate. Thus it is unlikely that even a shot from the 81-ton gun could penetrate such armour, and it would probably require several shots striking in succession on the same spot to make a breach. As yet this is only theory, but there is very little doubt that it would be confirmed by experiment. It would be well worth the cost of putting up a target at Shoeburyness and sending the 81-ton gun down there. Compared with the twelve-inch armour of the *Devastation*, it has been calculated that the strength of the armour of the *Inflexible* is as $2\frac{1}{4}$ to 1; but the calculation has been based only on the thickness of the iron, the element of strength derived from its peculiar arrangement being left out of account, and the comparative resisting power of the armour of the *Inflexible* must be very much higher. With this immense mass of metal on her sides, with 18 inches of it on her turrets, and an armament of four 80-ton guns, her displacement is 10,866 tons, but she will have three feet less draught of water than the *Dreadnought*,* though that ship will carry only 14-inch armour and four 35-ton guns.

But the ram and the torpedo are now weapons perhaps even more formidable than the gun; and while her armour may be relied upon to protect the *Inflexible*, at least from any gun now afloat, ample care has been taken to obviate as far as possible the dangers of ram and torpedo attacks. The handiness of the ship with both her screws going will make it very difficult for

* The greatest draught of water of the *Dreadnought* will be between 26 and 27 feet. Until a few weeks ago and before her change of name the *Dreadnought* was known as the *Fury*.

a hostile ram to give her a fair blow, but should she receive injury her peculiar structure—by which she is divided into an under and upper portion, with numerous water-tight compartments—would keep her afloat even with a large breach in her side. In addition to the ordinary bulkheads running across the ship, she will have one dividing her amidships in the direction of her length, and separate bulkheads specially constructed to isolate and protect the engines and boilers. In all, she will contain no less than 127 water-tight compartments; but numerous as they are, care has been taken to plan them so as not to interfere with the working of the vessel. The bottom will be double, and divided into several cells, in order to prevent any extensive injury from the explosion of a torpedo.

The armament of the *Inflexible* will be composed of four of the heaviest guns ever constructed, of which the experimental 81-ton gun now being tested at Woolwich is the type. Fig. 2 is a sectional sketch of the gun, showing the arrangement of the wrought-iron coils welded round the massive central steel tube. This tube, which forms the core of the gun, is bored out of a solid ingot, which cost about 1,700*l*. The bore is 24 ft. long, and rifled from the muzzle to within a few feet of the base of the tube, where the unrifled portion forms the powder chamber. The greatest external diameter of the gun is 6 ft.; at the muzzle it is just 2 ft. in diameter. The full calibre of the piece will be 16 in. The experimental gun has as yet been only bored out to 14½ in.; for the second series of experiments it will be given a calibre of 15 in.; it will then be bored to the full calibre of 16 in. and finally tested. Meanwhile the four guns which are actually to be mounted in the turrets of the *Inflexible* are in process of manufacture at Woolwich Arsenal.

The following are the approximate weights of the charges and projectiles for the various calibres of the 81-ton gun:—

Calibre. inches.	Charge. lbs.	Projectile. lbs.
14½	220	1,250
15	250	1,350
16	300	1,650

The 16-in. shell of the 81-ton gun will weigh nearly three-quarters of a ton; the bursting charge will be about a hundred pounds, that is, a whole barrel of gunpowder. Only the first series of experiments (those with the calibre of 14½ in.) is complete, and the gun is now being bored out to 15 in. The experiments occupied four days—Sept. 16, Nov. 18, and Dec. 9th and 10th, and gave excellent results. The coils and the steel tube bore the enormous strain without receiving the least injury or displacement, while the gun gave a very high initial velocity to its projectile, and there is little doubt that at 1,000 yards it would penetrate 20 in. or even 24 in. of armour. At 250 feet the flat-headed bolts used in the experiments were buried 50 ft. in the sandy butt at the end of the proof-ground. The experiments have shown, too, that the action of the pebble powder is perfectly under control; that by using the various sizes a greater or less velocity can be given to the shot, a greater or less strain brought to bear upon the gun. By means of Major Maitland's gas-check all windage is stopped, and thus none of the force of the powder is wasted, and the bore and rifling of the gun is saved from erosion by the gas escaping round the shot, which wears out muzzle-loading guns more rapidly than any other agency. Maitland's gas-check consists of a plate of copper with a heavy rim attached to the base of the shot. On firing the charge the soft copper is forced into the grooves, and stops the windage. This gas-check is now in use throughout the whole of our heavy artillery. The flat-headed projectiles used in the trials of the 80-ton gun weighed, including the gas-check, about 1,260 lbs. The powder used in the charges was the large cubical pebble powder described in the POPULAR SCIENCE REVIEW in January last.* The following table shows the velocities and maximum pressures in the powder chamber on each occasion:—

Round.	Charge. lbs.	Description of Pebble Powder.	Initial velocity. Feet per second.	Pressure on gun. Tons per sq. inch.
		September 17, 1875.		
1	170	1·5 inch cubes	1,393	24·2
2	190	1·5 " "	1,423	22·3
3	210	1·5 " "	1,475	24·8
4	220	1·5 " "	1,503	22·2
5	230	1·5 " "	1,550	29·6
6	240	1·5 " "	1,551	27·3
		November 16, 1875.		
1	220	1·5 inch cubes	1,525	25·8
2	220	1·7 " "	1,420	20·6
3	230	1·7 " "	1,454	20·2
4	240	1·7 " "	1,470	21·0
		December 9, 1875.		
1	220	1.5 inch cubes	1,535	24·1
2	220	1·7 " "	1,502	23·0
3	220	2·0 " "	1,485	21·7
4	230	1·7 " "	1,543	24·9
5	230	2·0 " "	1,498	23·4
6	240	2·0 " "	1,513	23·0

* Article, "Gunpowder, its Manufacture and Conveyance." By A. Hilliard Atteridge. January, 1875.

December 10, 1875.

Round.	Charge. lbs.	Description of Pebble Powder.	Initial velocity. Feet per second.	Pressure on gun. Tons per sq. inch.
1	220	1·5 inch cubes	1,440	28·1
2	220	1·7 " "	1,414	25·1
3	220	2·0 " "	1,366	24·4
4	250	2·0 " "	1,523	24·8

Rounds 1, 2, 3 of December 10 gave exceptional results, as they were fired with a projectile of 1,460 lbs. and consequently the velocities obtained were much lower and the pressures proportionally higher than with the smaller projectile ordinarily employed. It will be observed that pressure and velocity increased with the weight of the charge, but decreased as the size of the pebbles was augmented, the pressure, however, decreasing in a much greater ratio than the velocity. Thus the action of the charge is completely under control.

It has been assumed that it will be well to keep the pressure of the gas in the powder-chamber below 25 tons per square inch; and these experiments show that this can be easily accomplished, while at the same time giving a very high initial velocity to the shot. When the gun is bored out to its full calibre, it will probably give even more striking results.

Mounted on the ordinary carriages and slides, and worked by manual labour, these huge guns would be almost unmanageable, and at best would deliver only a slow inefficient fire. But all difficulties in the way of using them with good effect have been removed by an invention of Mr. Rendel, of the Elswick Works, which will make these monster pieces of artillery more handy than even the old 68-pounders.

The leading features of the arrangement are shown in fig. 3. Two guns will be mounted side by side in each turret. Each gun will be mounted so as to be supported on three points. The trunnions will rest on blocks sliding on fixed beams bolted down to the floor of the turret, while the breech will rest on a third block, sliding like the others between guides, upon a beam or table. Behind each of the trunnion-blocks, in the line of recoil, are two hydraulic cylinders, connected with them by piston-rods. The cylinders communicate by a pipe, on which there is a valve, that on the recoil of the gun opens and allows the pistons of the rams to run back slowly, checking the recoil. By reversing the apparatus, the gun can be run out again. The beam on which the breech rests is supported by a third hydraulic ram, fixed vertically beneath it in the turret. By this means the breech can be easily raised or lowered, thus elevating or depressing the muzzle of the gun, which pivots on its trunnions with a large preponderance towards the breech.

In order to load, the muzzle is depressed until it comes opposite to an opening made in the upper deck before the turret, and protected by a sloping armoured glacis. A hydraulic rammer works in guides through this hole, and the rammer-head is hollow, and is so constructed that when it is driven into the recently fired gun, and comes in contact with the sides of the powder-chamber, a valve opens, and it discharges through a number of holes small jets of water, thus acting as a sponge, and extinguishing any remnants of the charge or of the products of the explosion which may have remained smouldering in the bore. It is then withdrawn, and a hydraulic shot-lift raises up to the muzzle of the gun the charge, the projectile, and a retaining wad, and then a single stroke of the rammer drives them into the gun and home to the base of the bore. Again the rammer is withdrawn, the hydraulic ram under the breech of the gun elevates the muzzle, the turret swings round, and the shot is fired. A 9-inch gun, mounted experimentally in a turret at Elswick, and loaded on this system, was brought to the loading position, sponged, loaded, and brought back to the firing point in twenty-three seconds. Equally rapid loading was effected with the 38-ton gun during the experimental trial of the hydraulic gear on board the *Thunderer*. Thus the first advantage of the system is rapidity of fire; the second is economy of labour. One man only for each gun is stationed in the turret, another works the hydraulic rammer on the main deck, six or eight others are employed in bringing up the ammunition to the shot-lift by means of a small tramway. There are two sets of loading gear for each turret; but even if both were put out of order, the gun could still be loaded, with an ordinary rammer and sponge, by a number of men stationed on the main deck.

The adoption of the system enables very heavy guns to be carried in comparatively small turrets. Those of the *Inflexible* are very little larger than those of the *Devastation*; so that with the old plan of having a numerous crew in the turret, and running in the gun in order to load it by hand, only the 38-ton gun could be carried. As it is, it is quite possible that the *Inflexible* will be armed with even a more tremendous weapon than the 81-ton gun. This has been held in view in designing the ship; and, by a slight modification, it will be possible to mount in each of her turrets a pair of 160-ton guns, with a bore of 30 feet and a calibre of 20 inches.

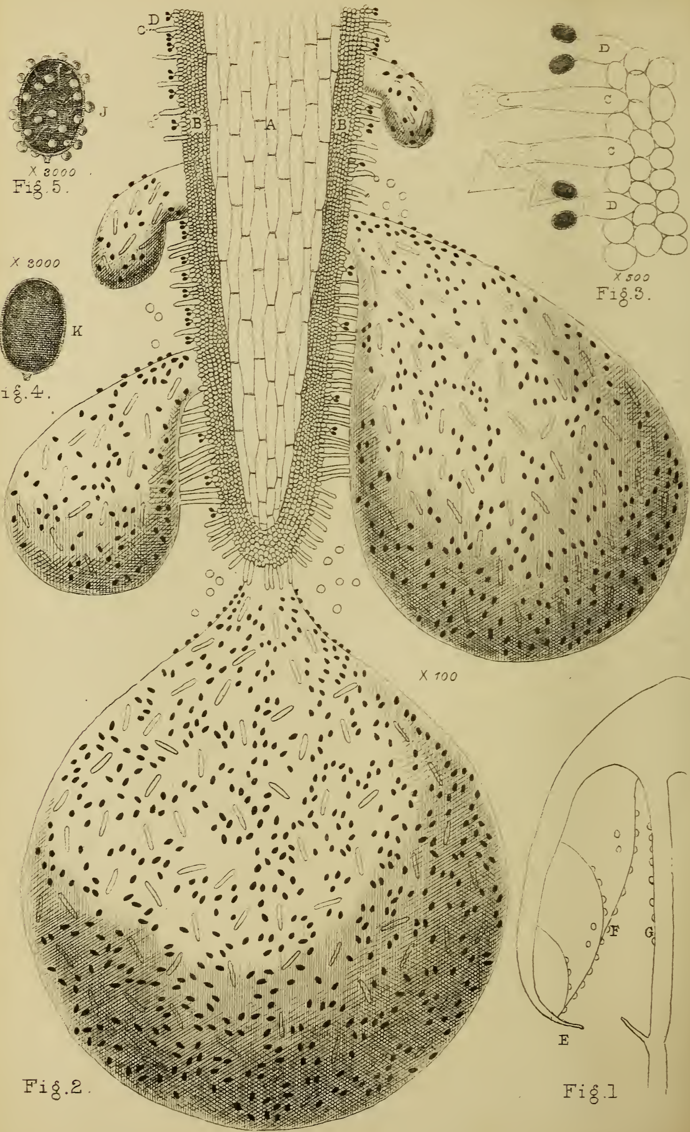
A minor feature, which will perhaps be introduced in connection with guns of large calibre, is a steel plug containing within it a detonating apparatus for firing a charge of powder. This is intended to be fixed in the vent of a heavy gun, in order to prevent the upward escape of the gas and the consequent gradual erosion of the vent. This erosion very rapidly widens the vent, and at last disables the gun, and the fire has to be suspended until it is revented. Thus this system of firing, which has been invented by Capt. Noble, R.A., would greatly increase the efficiency of the gun.

Some idea of the amount of ammunition required for the 81-ton gun will be given by the following calculation :—Let us suppose that in an action the *Inflexible* would fire only ten shots from each of her guns; she would use up more than 1,300*l.* worth of ammunition, burn upwards of 100 barrels of pebble powder, and hurl nearly thirty tons of iron at the enemy.

As a new type of man-of-war, we may sum up the leading features of the *Inflexible* as follows :—The armour is confined to the central fighting portion, and to the main substructure which floats the ship. An armoured deck, seven feet under water, divides the vessel into two separate portions. The unarmoured ends are so constructed that the vessel will float even when they are penetrated. The ship has a wide beam and a comparatively light draught. The deck-houses give a high bow and stern, and the turrets are so arranged as to enable all four guns to be fired both ahead and astern, or on either beam. The *Inflexible* has been accepted as the type of our future line-of-battle ship; a few years may perhaps introduce into naval warfare such changes as to render the principles on which she has been constructed obsolete. But with our present knowledge no better design could be adopted, and already the Government has determined on immediately laying down two new ships of the same type, but of smaller size. They are to be called the *Ajax* and the *Agamemnon*. Their displacement will be about 8,000 tons—that of the *Inflexible* is 11,000. They will carry 18-inch armour on the central section, and two 38-ton guns in each of their turrets.

NOTE.—The deck-plan of the "Inflexible" is only a rough sketch intended to show the arrangement of turrets, decks, &c., in order to obtain a fire, ahead, astern, and on either beam, for all four guns.

Plate CXXXII.



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Reproduction of Agaricus lacrymabundus.

HOW MUSHROOMS ARE REPRODUCED.

(*Agaricus lacrymabundus. Fr.*)

By WORTHINGTON G. SMITH, F.L.S., M.A.I.
F.R.Hist.S. Ireland, &c.

[PLATE CXXXII.]

POSSIBLY there is no branch of fungology so little understood as the reproductive process in the *Hymenomycetes*, of which family our common mushroom is a good example. Professor Sachs, in his recently published "Text-Book of Botany," in referring to the *Basidiomycetes* (of which the *Hymenomycetes* is one family), says: "Although the largest and most beautiful fungi belong to this order, yet their course of development is at present only very imperfectly known. In contrast to the variety of form occasioned by the alternation of generations in most other fungi, and to the singular phenomena of the mycelium of the *Ascomycetes*, it is remarkable that similar processes have not yet been established in this class. The origin from the mycelium of the usually large receptacles, and their further development, are known in their more conspicuous features, as is also the mode of germination of their basidiospores; but the history of the mycelium before it forms the receptacle is still unknown. "I must therefore," says the professor, "content myself with a few morphological explanations of the development of the latter in the most striking forms of the *Hymenomycetes* and *Gasteromycetes*." Unfortunately for Professor Sachs, the English editors of the "Text-Book" refer to Œrsted's observations of the reproductive process in *Agaricus variabilis* as reproduced in the "Quarterly Journal of Microscopical Science" for 1868, p. 18. As Œrsted founded his conclusions upon mycelium gathered from a mushroom-bed where *A. variabilis* is (as far as my experience goes) never found, his deductions lose a great deal of their value. Indeed, nearly the whole subgenus to which *A. variabilis* belongs is peculiar to decayed wood and moss. Formerly *A. variabilis* was included in the subgenus Crepidotus, but Professor Elias

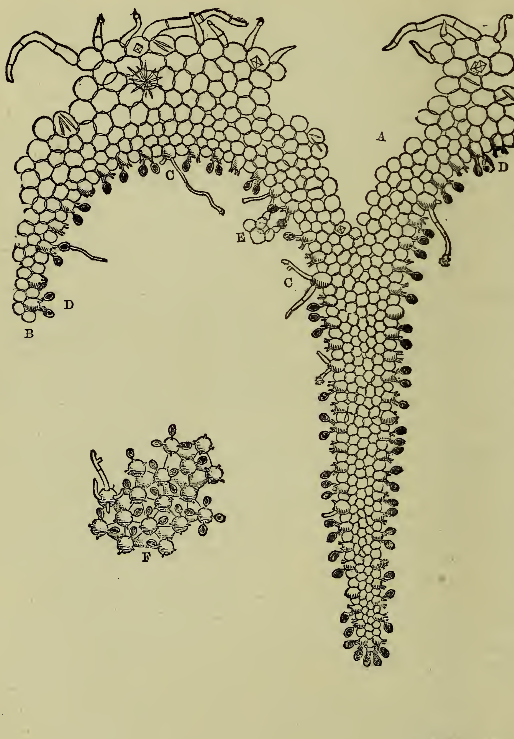
Fries, of Upsala, following the views not long since published by the writer of these lines, now places the species in a new subgenus of Agaricus,—Claudopus (*Hymenomycetes Europæi*, *Epicriseos*, p. 213), and says, under *Agaricus* (*Claudopus*) *variabilis*: "Ad ligna et truncos emortuos, præcipue Abietis."

In illustration of the hymenium, or reproductive surface of the gills in the *Hymenomycetes*, Professor Sachs gives a figure of the minute structure of the common mushroom (*Agaricus campestris*). But, unfortunately, the figure and description alike are far from correct. Sachs says the basidia in this species produce only two spores, whilst in other *Hymenomycetes* the number is usually four, and the illustration is made to accord with the description. But the fact of the case is that there *are* four spores produced in each basidium in *Agaricus campestris*, and this fact does not apply to *A. campestris* alone, but to every variety of it, and every variety of its numerous allies, of which the common horse mushroom (*A. arvensis*) is one. The nature of the hymenium of the common mushroom is correctly figured by Dr. M. C. Cooke, in the "Popular Science Review" for Oct. 1869, Pl. LIII. fig. 14, where each basidium is shown, furnished with four (not two) spicules. Each of these spicules normally bears a spore, but it is a common thing in Agarics for the four spores to be produced two at a time, diagonally; as the first two spores become ripe, two other and younger spores appear on the spicules at right angles with the first, and the two latter push the two former off. Sachs was evidently unacquainted with this fact. Seeing only two spores at a time on the basidia of the mushroom, he overlooked the fact that two had already been pushed off, or were not yet produced. It is, however, quite common to see all four spores produced at the same time in the mushroom, so that there is not the slightest foundation for reducing the basidia in *Agaricus campestris* to the production of two spores only. Le Maout and Decaisne, in their "Descriptive and Analytical Botany," p. 953, correctly figure the basidia in *A. campestris* with four spores; but, unfortunately, the description of reference to the basidia and the analogous organs (cystidia) is far from correct. These authors are still more incorrect under their description of *Geaster hygrometricus* (p. 956), one of the starry puff-balls, and, like the mushroom itself, one of the *Basidiomycetes*. This species is described by Le Maont and Decaisne as a hypogeal globose plant, which, they say, presents the following curious phenomenon: "when mature and still underground, if the season be dry, the outer envelope, which is hard, tough and hygrometric, divides into strips from the crown to the base; these strips spread horizontally, raising the plant above its former position in the ground; on rain or damp supervening, the strips return to their former position; on the return of the drought, this process is repeated, until the Fungus reaches the surface, becomes epigeal, and spreads out there; then the membrane of the conceptacle opens to emit the spores in the form of dust." This description is altogether incorrect; in fact, just the opposite state of things holds good, for the strips become horizontal from moisture, and are rigidly inflexed when dry. Fries pointed this out, in his "Systema Mycologicum," nearly fifty years ago. These errors, in our best and most recent text-books, are here pointed out, not from any desire to find fault or detract in the slightest degree from two thoroughly excellent works, but simply to show how difficult it is to make new and correct observations. In the following paper and the one recently published by me, on the reproduction of *Coprinus radiatus*, in the "Gardeners' Chronicle" for Oct. 16 and 23, 1875, I have, to a certain extent, broken fresh ground. My views, although long since foreshadowed by various competent botanists, are yet different from those now generally held regarding the reproductive process in the *Basidiomycetes*. If I am in error as to my supposed facts, or the deductions I draw from them, I am at least in good company, and I have not warped any of my observations to fall in with the previously expressed views of any other writers whatever.

The nature of the gills in various members of the mushroom tribe differs very much, and this difference is especially remarkable in the absence or presence of the trama, and in the number, form, and size of the cystidia. In the genus Coprinus there is no trama to the gills. The trama in true Agarics is formed by cells, which are of a different nature from the other simple cells which go to form the gills, and this trama forms the intermediate substance between the hymenial surfaces.

Nothing can be more different than the interior structure of the gills in the genus Coprinus, and in *Agaricus lacrymabundus* (Fr.). In fig. 1 is shown, enlarged 150 diameters, a vertical transverse section down a gill of *C. radiatus* (Fr.), which species is (though minute) quite typical of the entire genus. The trama with its large cells would, if present, be at A; but a glance at the figure will show that the individual cells throughout this section are almost identical in size. If reference is now made to the similar section of a gill belonging to *A. lacrymabundus*, Fr. (and shown on Pl. CXXXII.), the nature of the cells at A, which go to form the trama, will be quite apparent. Wide differences of cell-form like this are common in the mushroom tribe, but the difference of form as found amongst the cystidia is still more striking. In *Coprinus* *atramentarius* (Fr.) the cystidia are indeed so large that each individual cystidium would hold within itself more than 300 of the ordinary cells which go to build up the pileus. These facts

Fig. 1.



Coprinus comatus. Fr.
Vertical section and surface (F) of Gill enlarged 150 dia.

regarding the cap and the hymenial surface (to say nothing of the gills often completely separating from the horny hymenophorum, as in Paxillus), and many other known facts in regard to the nature of the stem and its outer surface, go far to disprove the hypothesis that the higher fungi found under the Agaricini may be generalised as mere concreted masses of moulds. No bringing together into a mass of a forest of Penicellium could even make an Agaricus, and to me there is nothing in common between the conidio-spores of Penicellium and the basidio-spores of the Agaricini.

The various bodies which, as I believe, belong to the reproductive process in the mushroom tribe are seen in figs. 1 and 2 and Pl. CXXXII. The peculiar cells of the trama are shown at A, Pl. CXXXII., the simple cells which form the external and hymenial surface at B, and the privileged cells at C, D. C shows the cystidia and D the basidia. Each basidium in the Agaricini bears four minute spicules, which carry the same number of spores. The difference in size between the basidia and cystidia is often immense, and in *Agaricus lacrymabundus* the perfect forms of the two organs are very different from each other; they, however, so approach each other in this species, that every intermediate form between a cystidium and a basidium may be observed. In *Coprinus radiatus*, as I have shown elsewhere, the cystidia have their contents differentiated, and at once produce spermatozoids; or they germinate, and the spermatozoids are produced from a differentiation which then takes place at the end of the thread; these spermatozoids attach themselves to and ultimately pierce the spores, and their piercing causes the discharge of a single cell from the pierced spore, which cell belongs to the pileus of the new plant. As I interpret my observations, this process is carried on upon the gills themselves, as shown in fig. 1, and at the time of the perfect maturity of the fungus, at least in Coprinus, in the subgenus Volvaria, in the ordinary mushroom, and in such plants as have fallen under my notice. The basidia with their spicules and spores, the cystidia germinating and bearing spermatozoids, and the first cells of a new plant (E), are all shown in position in fig. 1.

Agaricus lacrymabundus (Fr.) is a very different plant from *Coprinus radiatus*. It is one of our commonest Agarics of the autumn, and is usually found in damp pastures and about stumps. It bears a considerable resemblance to the common mushroom, and is without doubt often gathered for the table in mistake for that species, but whether with any ill effects or not I am unable to say. The most striking character of this Agaric resides in its gills, which are always furnished with a white edge, which drips with semi-milky tear-like drops. Why this singularly good character is not given by Berkeley in his "British Flora," or his "Outlines of British Fungology," or by Dr. Cooke in his more recent "Handbook," I cannot say. Indeed, Fries himself (whose species it is) does not advert to this salient character in his "Systema Mycologicum," or in either editions of his famous "Epicrisis." In the "Monographia Hymenomycetum," Fries, however, says, in reference to the gills of this plant, "acie albicante et jove udo plorante."

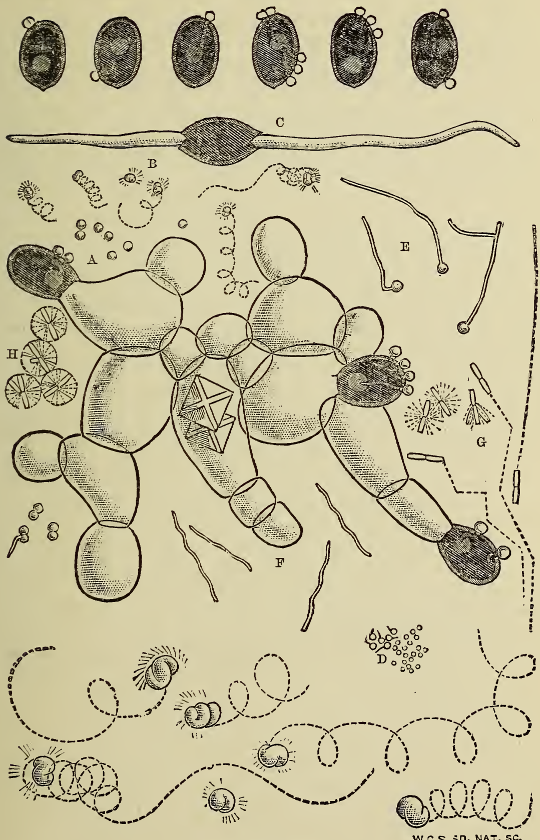
Part of a section of a specimen of *Agaricus lacrymabundus* is shown at E, Pl. CXXXII., with the characteristic drops *in situ* on the gills and upper portion of stem, F G. The drops are shown towards the edge of a gill enlarged 100 diameters at H, above which may be seen several drops of a smaller size, which may or may not at length coalesce with the lower drop. These drops invariably dry up on the edges and surface of the gills. As far as I know, milky tears peculiar to *Agaricus lacrymabundus* have never been minutely described, neither has an account of any microscopical examination of them been hitherto published.

These drops are distilled direct from the cystidia. When the fungus reaches maturity the cystidia protrude boldly from the surface of the hymenium, as seen at C; and the apex of the cystidium opens exactly as does the ascus in Peziza. At first the cystidium is filled with a thin fluid in which no granules can be detected; but at length a large vacuole is formed at the base of the cystidium, and the contents are forced towards the apex: here the fluid becomes partly differentiated into granules, and at length the cystidium opens at the top and discharges its contents. The protruded drop now swarms with moving atoms of a regular size, which I refer to spermatozoids. They are in every way identical with the spermatozoids of *Coprinus radiatus* (Fr.).

In my recent paper on the reproduction of *Coprinus radiatus* I adverted to the fact of the cystidia falling bodily away from the hymenium, and exactly the same phenomena holds good in *Agaricus lacrymabundus*; for if a drop of the liquid from the gills is examined under the microscope it will be seen to swarm with free cystidia which have dropped into the fluid from the gills; this liquid not only abounds with spermatozoids and systidia, but it also swarms with spores which have fallen away from the basidia, as seen in Pl. CXXXII.

If the spores which have fallen into the drop are examined, they will be found to be very different in aspect from the spores as they are seen upon the basidia; for whilst the spores in the latter position are perfectly plain in outline, as seen at J, enlarged 2,000 diameters, the spores within the drop are studded all over the surface with the spermatozoids discharged from the cystidia. Not only are the spores studded as shown at K, but they are pierced through the external coat by a fine thread protruded from the spermatozoid. The effect of this is that a single free cell is soon after discharged from the spore, and this

Fig. 2.



Coprinus radiatus. Fr.

Spores pierced by Spermatozoids ; unpierced Spore germinating at both ends ; infant plant and infusoria enlarged 1,000 diameters ; Spermatozoids at bottom of plate further enlarged 3,000 diameters floats about in the drop, leaving the exospore quite empty. This single transparent cell, which is three or four times the size of the original spore, and is the first cell of the pileus of a new plant, is in its turn commonly attacked and pierced by the spermatozoids. Sometimes a spore is surrounded and pierced by a large number of spermatozoids, whilst at other times it receives the attack of one body only ; this is, however, sufficient, for I have frequently seen the first cells discharged from the orifice made by a single spermatozoid, and the latter body again pushed out by the cell. The spermatozoids which have a single turn of a spiral revolve rapidly, and, when they come in contact with a spore, not unfrequently creep with an amœba-like movement over the surface, before they discharge their contents into the protoplasm of the spore. These bodies, as seen in *Coprinus radiatus*, are shown in the accompanying figure, enlarged to 1,000 and 3,000 diameters, and they are precisely the same in *Agaricus lacrymabundus.* When the spermatozoids of the latter species emerge they are dark brown in colour, and here they resemble the spores ; the at first colourless contents of basidia and cystidia alike become at first differentiated and then oxidised on exposure to the air. In a minute species like *Coprinus comatus*, where the growth of the plant is very rapid, and hundreds of generations are produced in one season, new plants are rapidly reproduced from the spores, and the growth can be watched; but in *Agaricus lacrymabundus* the case is very different, for it is probable that it would take many months (if not a whole year) to grow a perfect fungus belonging to this species from the spores. It must also be remembered that although this Agaric produces many millions of spores, yet the plant does not increase in actual numbers ; for if all the spores produced plants, then in one year the world would be covered with this species only. For one spore which grows, hundreds of millions must be failures. In a former paragraph reference was made to bodies intermediate in form between cystidia and basidia, and bodies of this nature are common on the upper part of the stem in *Agaricus lacrymabundus* ; they are shown on the natural size section at G. The bodies in this position secrete and distil a semi-milky fluid, which they excrete in drops, which drops are invariably dotted all over the upper portion of the stem. It must not be thought that this distillation is peculiar to the species under notice, for it is a character (though till now unpublished) of whole sections of Agarics. For instance, certain groups coming under Tricholoma are described by Fries as " lamellis decolorantibus, rufomaculatis l. cinerascentibus." Now these brown spots on the gills, when examined under the microscope, are seen to be produced by a liquid which changes colour, and which is distilled from the cystidia; and it is quite possible that the same phenomenon holds good in many other fungi, as in the tears of *Merulius lacrymans*, and in the drops found upon such species as *Polyporus dryadeus*, *P. hispidus*, *P. cuticularis*, and many others. It seems probable that in some species this liquid, which by a differentiation produces spermatozoids, also sends the spores into a temporary resting condition, and that the spores rest before germination, just in the same way as many seeds rest. This would explain the great difficulty of getting some fungus-spores to germinate. Under any circumstances diverse fungus-spores resemble seeds in the fact of some germinating at once, whilst others will not germinate till long periods of time have elapsed.

On examining the semi-milky drops as seen on the stem, I have been unable to observe any production of spermatozoids, but these intermediate cystidium-like bodies are expelled from the stem and fall into the drops of moisture distilled. This is a state of things to be expected, for as these bodies belong to neither basidia or cystidia, they are as a consequence quite as unable to produce spermatozoids as spicules and spores. In Agarics and Boleti the stem may be considered as a mere barren hymenium; when the stem is striate (as it commonly is), these striæ represent the absent gills; in species with decurrent gills, the fruiting surface (as in *Agaricus prunulus*) sometimes quite reaches the ground. When the stem in Boletus is reticulated, the reticulations represent the absent tubes, so that it should be a matter of no surprise to find organs pertaining to the hymenium upon the stem. The external surface of the pileus, for the same reason, often bears besidia and spores, and the stuffed stem and cartilaginous bark answers to the trama and hymenial surface. An Agaric or Boletus with the pileus and gills arrested would answer to a Clavaria bearing fruit all over the club, and abnormal Agarics are sometimes found in this condition, whilst the simplest form of *Hymenomycetes* is where a merely filmy hymenium is developed, as in Hymenula.

When it is remembered how innumerable are the myriads of spores and spermatozoids set free every autumn, and how probable it is that hybrids of every degree are produced from these bodies, the diverse and almost countless forms seen in the mushroom-tribe quite cease to be a matter of wonder. Without doubt the spermatozoids of some species commonly pierce the spores of an ally, and so produce plants intermediate between one species and another; such forms are an everyday experience with fungologists and lichenologists, and the more one knows of species as species the greater are the difficulties to be surmounted in correct naming. Fries himself says, under Mycena, that he has only given the best marked species, and that he knows in his mind many other apparently distinct forms and species which he has never published. Our cryptogamic floras require cutting down to at least one-fourth of their present dimensions by some competent botanist, who is well acquainted with species. In the majority of instances the species are so ill defined that it is the easiest possible matter to gather plants which it is simply impossible to refer to any described fungus, simply because the specimens gathered belong to none.

The spore, as found amongst Agarics, I consider female in the sense of its pertaining to the female in the same way as an unimpregnated ovum may be considered a female organ whilst still attached to the mother, and the spermatozoids male, as pertaining to the male in the same way as spermatozoids are always the direct offspring of the male (as ova are of the female). When the spore is pierced by the spermatozoid, the former is capable on germination of producing either or both sexes. If these views are correct, a term is required for the unpierced spore synonymous with the ovule in flowering plants, as distinguished from the seed; and the old term sporule might well be used for indicating the unpierced spore. The spores in *Agarics* must be considered very different in nature from gemmæ, or buds, and they are totally different from the conidia or conidio-spores of Penicillum and the acro-spores of Mucor. The latter may reasonably be compared with the bulbils of Dentaria, and some Liliaceæ; but the nature of the basidio-spores (and of the hymenium which carries them) points to a very different conclusion. It is a very common thing for the mycelial threads of all sorts of fungi to break up into bead-like conidia, and as these conidia or secondary spores have a certain reproductive power, there is here certainly a slight analogy with gemmæ. The spores and spermatozoids in the Agaracini appear to me to have a strong analogy with the oosphere and spermatozoids of *Fucus vesiculosus*, the cystidia having an analogy with the antheridium in the same Alga. I have expressed an opinion elsewhere that Van Tieghem's idea of male and female spores in the Agaricini is altogether untenable; such a thing as a male ovum or spore is as unreasonable as a female spermatozoid or female pollen-grain. Seeds of all sorts are capable, on germination, of producing either or both sexes, though it is common enough to see one sex exalted at the expense of another. Even in the highest mammals the males have a trace of the female in the subordinate mammæ and other characters, and similar characters which show a trace of the male are found in most female animals.

In the vegetable kingdom nothing is more common than to find so-called male or female plants changing their characters. Males will, under altered conditions, carry female organs, and females will produce anthers and pollen, which conclusively shows that not only are ovules, spores, seeds, and eggs not in themselves male or female, but the produce itself of these eggs is inherently hermaphrodite. It is convenient to name many animals and plants "male" and "female," because they are almost but not entirely so. Even in the case of the Equisitaceæ, Sachs is obliged to qualify his terms of regarding these plants, and say that on the germination of the spores "the prothallia are, *in general*, diœcious" ("Handbook," p. 363). And on Ferns the same qualification of terms is found, for under the latter head he says (p. 343): "The prothallia *show a tendency* to be diœcious."

Several papers have been published of late on the reproductive process in the *Basidiomycetes*, and the writers of these papers have noted (with me) the diverse mycelia seen amongst the germinating spores; but I am convinced that the threads which produced antherozoids, as seen by Van Tieghem, came from the cystidia and not the spores. When a mass of spores and threads are seen in a solution of horse-dung, nothing is more difficult than to decide for certain whether the threads really come from the spores or not, and the spores of *Coprinus radiatus* could not possibly have been "sown" on any decoction without cystidia likewise falling upon the liquid. The "rod-like" bodies described by Van Tieghem read remarkably like an illustration of the well-known fact of the threads of many fungi breaking up into Bacteria at the tips. In the many Agarics I have examined, it invariably happens with me that after a spore is pierced it discharges a single cell, which developes directly into a new individual, exactly as is seen in Chara, but an unpierced spore may produce a thread of indefinite length; now if this thread is attacked by the spermatozoids, it will in its turn produce this single primordial cell of a new fungus, or if the mere undifferentiated liquid contents of the cystidium should pass over the thread from an unpierced spore, it there gives rise (at the point of contact) to the primordial cells.

The persistence of form in spores, and especially the pierced spores, under extraordinary conditions, is something remarkable; for instance, repeated violent boiling has no effect on the form and colour of the spore, and in the cases where the spermatozoids are attached no amount of boiling disengages them; if anything, the boiling seems to make the piercing more distinct to the eye. I do not find conidia or gemmæ resist boiling in this manner, and certainly no bulbil or bud from a flowering plant would maintain its form under these conditions. As for the unpierced spores, I believe their life to be of the very shortest duration (not twenty-four hours), but after piercing the life remains, just as we find life slight in an ovule but enduring in a seed.

Dr. Max Reess has recently published an account of *Coprinus stercorarius* (which is a rare species and not British) in Pringsheim's "Jahrbücher," in which he has described and figured a sort of Carpogonium (or Ascogonium), a female organ said to be produced at the ends of mycelial threads. This organ, according to Dr. Reess' figures, would appear to be furnished, or not furnished, or doubly furnished at the apex with a trichogyne, an organ into which the spermatozoids discharge their contents. This trichogyne, according to Sachs in describing the Algæ, is "a long thin hair-like hyaline sac, which serves as a receptive organ, and springs from a structure which is called the trichophore. This latter is a body usually consisting of several cells, in or near which the results of the fertilisation become apparent, cell-filaments or masses of tissue shooting out near or beneath it, forming the receptacle, here termed the cystocarp, in which the spores are subsequently formed. In one genus (of Algæ), *Dudresnaya*, the process is still more complicated, inasmuch as the tubes first spring from the trichophore, which occasion the formation of the cystocarps only after conjugation with the terminal cells of other branches." The Carpogonia, as figured by Dr. Reess, remind me of a number of spores agglutinated together with spermatozoids still attached, exactly as similar agglutinated companies of spores may be always washed from the dry brown spots on *Agaricus lacrymabundus* or the various brown-spotted Tricholomata. I am disposed to fall in more or less with Dr. Reess' views, although I do not know the species he has worked upon. The single primordial cell which I have seen expelled from the spore of *A. lacrymabundus* and then pierced by spermatozoids before going into a resting state must be of the nature of a Carpogonium. The spores commonly put on an appearance similar to that figured by Dr. Reess, but anything referable to a tricogyne I have not seen.

Dr. Eidam has also recently published an illustrated paper on the reproductive process in the Agaricini in the "Botanische Zeitung" for the 1st and 8th Oct. last. Unfortunately, Dr. Eidam's paper has reference to *Agaricus coprophilus* (Bull), a rare Agaric in this country, and one almost unknown to me. Here, at fig. 12, Pl. VIII., a Carpogonium is again suggested, but Dr. Eidam very properly and reasonably attaches a note of interrogation to his interpretation of the figure. At the ends of certain threads Dr. Eidam figures what he terms spermatia, which, though somewhat different in shape, are identical in size with the bodies I term spermatozoids. My spermatozoids are at first globular, fig. 2 A, then somewhat elongated from the unfolding of one turn of a spiral B; and these bodies, with me, although often produced direct from the cystidium itself, are quite as often produced at the end of a long thread given out from the cystidium. With me, I find the unpierced spores to burst at one or both ends, fig. 2 c, exactly in accordance with Dr. Eidam's figs. 1, 2. The ruptured end of the exospore from which the threads emerge are shown in my fig. 2 c, and are uncommonly well shown in Dr. Eidam's figs. 1, 2. In Dr. Eidam's plate the two orders of threads are well shown, but I am perfectly convinced that the second order of mycelial threads which bear the spermatia do not belong to the spores. These latter threads are shown by Dr. Eidam to a larger scale than the threads from the unpierced spores, and there are only three spores shown in connection with the spermatia-bearing threads. Now, although these three latter spores are shown to nearly double the scale of the other spores, yet in each case *the wall is shown unbroken.* This is simply because the walls belonging to these spores are correctly shown in the plate; the threads do not come from the spores (which have been merely accidentally washed against the threads), but from the fallen cystidia. In this plate there are no less than sixteen spores producing threads without spermatia, and *in every instance* the spores are then shown as *ruptured.* I am quite disposed to acquiesce in the correctness of Dr. Eidam's observations as regards the spermatia, but the spermatia-bearing threads are from the cystidia. Fig. 12 can hardly be a Carpogonium; it disagrees with Dr. Reess' figure and description, and quite accords with what I have seen of the terminal condition of a cystidium-thread when the contents are about to become differentiated. My impression is that any Carpogonium in the Agaricini must be a pierced spore or an agglutination of pierced spores. These spores or companies of spores may rest for longer or shorter periods, and the cells and mycelium when at length produced is then analogous with a subterranean rhizome, capable of producing new plants from buds.

It will be observed that the notes on both the species last referred to, as well as my notes on *Coprinus radiatus*, bear upon dung-born species of rapid growth. As dung swarms with fungi and infusoria of all sorts, the greatest care is necessary in experiments, or Bacteria will be mistaken for Spermatia, and Sphærobacteria for Spermatozoids. To clear up some little of the confusion which might arise as to all these bodies, the infusoria are engraved in fig. 2 to the same scale as the spores and bodies referred by me to spermatozoids; A shows the globular spermatozoids as they are at first produced within the cystidium, or within the end of a thread more or less long, protruded from a cystidium: these bodies are at first motionless, but after being kept in liquid for a few hours they begin to slowly revolve; this movement keeps on for several days, and is all the time accelerated, whilst the body (formerly spherical) now becomes slightly elongated, and a single turn of a spiral is seen as in the larger figures shown at the bottom of the plate; the dotted lines indicate the whirling motion of the spermatozoids. For comparison with this, various tailed and tailless monads are engraved at D, which figures may be compared with the spermatozoids to the same scale on the upper part of the plate. These latter bodies have been long known, and Mr. Berkeley, in the "Micrographic Dictionary" (last edition), p. 20, says: "The bodies called cystidia or pollinaria are globular or oval cells, found associated with the basidia, containing granular matter, exhibiting molecular motion when discharged." In common with many other botanists, I am inclined to think this movement to be other than molecular, as the eddy clearly seen round the revolving bodies indicates the presence of cilia. It is impossible to believe that *life* can spring from *no-life*, the mere molecular movement of lifeless particles must be of a totally different nature from the life-movements of spermatozoids and infusoria. The revolving bodies might be referred to Micrococcus, one of the Sphærobacteria, but it must be remembered that they are the differentiated contents of the cystidia. It may be answered that if the mycelial threads of fungi will break up into true Bacteria, why may not the cell contents reappear as one of the Sphærobacteria? The question then arises, What is the nature of the obscure bodies referred to Bacteria? Are Dr. Eidam's spermatia no other than Bacteria? they are sufficiently like Bacteria. Are certain so-called Bacteria and antherozoids of some cryptogams one and the same? When once produced they are both very persistent, and they are both produced under somewhat similar conditions. The spermatozoids of Agarics only appear as the fungus has just passed maturity, and as decomposition is setting in; and as soon as the spermatozoids are set free, then the material from which they spring has vanished.

It is just worthy of note that certain spermatozoids and bacteria resemble each other in size and external form, that they are alike in being furnished with flagella or cilia, that they are produced at a time when decomposition is just setting in, and that they are seen in semi-milky fluids. The spherical bodies, when they become attached to the spores, or even threads, rupture at one end and discharge a fine thread, as at E, where they are shown germinating in a free state, which is no uncommon occurrence. Vibriones are shown at F; these bodies, with the bacteria, shown at G and H, are well known to all microscopists, and it has appeared to me that they are mere differentiated states of the old cells of the fungus which have broken up and (as a collection of cells) totally disappeared. The dotted lines indicate the movements of the bodies, whether straight, zigzag, or revolving like a wheel.

Whether the fertilised spores are able to withstand great vicissitudes of temperature and still not lose their vitality is unknown, but if other living atoms are able to live in boiling fluids, or be frozen and still live, it should be a matter of no surprise to find fungus-spores passing unscathed through similar ordeals. It is reasonable enough to imagine life to be maintained under extraordinary conditions, but to me most unreasonable to imagine life to spring from that which is without life.

As the cells of decomposing fungi disappear, various infusoria at the same time swarm into being and take the place of the collapsed cells. On violently boiling these infusoria in test-tubes along with fragments of putrid fungi, for five minutes, and sealing at the highest point of ebullition, I find the infusoria, after one or two or three months, to be still alive. During this time putrefaction is arrested in the test-tubes, the decayed fragments of fungi remain the same, and the infusoria remain inert. On opening the flasks the infusoria are at first motionless, and life is apparently at a low ebb, but the individual infusoria, as watched under the microscope, rapidly regain their accustomed activity, and in a few hours are as full of life as if they had never been boiled. The decomposing fragments of fungi now rapidly give the fluid an offensive odour.

Whilst able to resist the heat of boiling-point, these same infusoria are equally well able to resist cold, for during the frosts of the past month I froze decoctions of fungi containing infusoria into solid blocks of ice for a whole week. On thawing, after this time the bacteria, vibriones, monads, &c. were as full of life as before freezing.

EXPLANATION OF PLATE CXXXII.

Fig. 1. Vertical section through gills and stem of Agaricus (*Hypholoma*) lacrymabundus (Fr.). E, Veil. F, drops on gill. G, drops on upper part of stem, natural size.

Fig. 2. Vertical transverse section through edge of gill, with drops *in situ.* Enlarged 100 diameters. A, Trama. B, Cells forming Hymenium. C, Cystidium. D, Basidium. H, lowermost drop, containing cystidia and spores.

Fig. 3. Cells of Hymenium. C, Cystidia, containing spermatozoids. D, Basidia, with spores attached, and triangular flat crystals from the juices of the plant. Enlarged 500 diameters.

Fig. 4. Spore unpierced by spermatozoids, enlarged 3,000 diameters.

Fig. 5. Spore pierced by spermatozoids, from a drop which had dried on the surface of the Hymenium. Enlarged 3,000 diameters.

REVIEWS.

THE PHYSIOLOGICAL LABORATORY.*

THE progress of the study of Biology which has been made in this country during the past ten years is indeed vast, and it is, we should suppose, to a certain extent due to the various text-books that have been published and popular lectures that have been delivered, as well as to the movements made by the Universities, and the Government through South Kensington, that such a happy result has been achieved. But much as has been done, we fancy that the future looks brighter than ever, for we do not think we shall exaggerate its importance when we assert that the book which has been issued by Professor Huxley will do more to spread a general taste for biological pursuits than anything that has heretofore been published. For though the system described is of course that which has been followed in Professor Huxley's laboratory, yet we must remember, firstly, that in order to have access to that fountain-head the student must necessarily be living in London; and secondly, that even if he were a townsman, and a young one, he would have the extreme difficulty of surmounting those narrow prejudices which have been raised against liberal teaching by distinguished members both of the Churches of England and Rome. For it is idle to pretend that the discouragement which these two Churches have offered to free and non-sectarian scientific teaching have not been attended by the most injurious results. And it must be openly confessed, that great as has been the success of the admirable system of teaching carried out at South Kensington, it has not had that full and complete measure of beneficial results which would have accrued had it not been distinctly, if not overtly, opposed by the two sections of the religious world to whom we have already referred. It is, therefore, for these reasons that we believe that a book like that now published will do much toward the spread of natural science—and not mere bookworm cramming—for it will enable anyone who masters its details to say that he has got no inconsiderable knowledge of the science of Biology. We do not know why Professor Huxley did not issue the book with his own name alone upon the title-page, for we are sure that most of the work is from his pen. But, be

* "A Course of Practical Instruction on Elementary Biology." By T. H. Huxley, LL.D., Sec. R.S.; assisted by H. N. Martin, B.A., M.B., &c. London: Macmillan & Co. 1875.

that as it may, it is without doubt a piece of work novel in its kind and thoroughly well executed, and withal it is a work which cannot fail to make the space in the laboratory at South Kensington more sought after than ever.

And what is the plan of this book, it will be inquired? It is simply that which anyone who has gone through a little elementary course of training can readily master. It is constructed on a plan entirely *sui generis*, and it is intended to give the reader who works with it an accurate insight into Biology generally. Therefore it is, of course, a work which deals with both Vegetable and Animal life. It is divided into thirteen chapters, in each of which is discussed the following subjects, and the last section of each chapter is devoted to what is termed Laboratory work—*i.e.* to certain observations and experiments tried upon the animal or plant under examination, in order to make out practically its structures and functions. The first of these is on Yeast, which is dealt with very fully and very practically; and then follow the other subjects: Protococcus, Amœba, Blood-globule, Bacteria, Penicillium, Mucor, Stoneworts, the Bracken fern, the Bean-plant, the Bell-animalcule, the Hydra, the Fresh-water Mussel, the Fresh-water Crayfish, the Lobster, and last, but by no means least, the Frog. Very naturally the author has dealt more fully with the amphibian than any other animal or plant, because it is the only vertebrate animal dealt with in the book, and it is universally abundant, and is so easily chloroformed and then "pithed," so that pain is thus completely prevented. The preceding animals and plants occupy 151 pages of the book, while the frog takes up the remainder of the volume, minus three pages; that is to say, 106 pages are devoted to its descriptive anatomy, which is minutely dealt with. In fact, there is an elaborate account of the general anatomy of the frog, which is completely dissected, including its brain, nerves, muscles, and vascular tissues. And further, the author treats not only of the structure of the animal so far as it may be followed by the naked eye, but he deals also most fully with its microscopic anatomy, and, in this instance as in all the others, makes the laboratory work follow the anatomical description. And both of these sections are dealt with as those familiar with Professor Huxley's work can readily imagine for themselves. Whether it is the examination of the blood, or the preparation of the vascular system for dissection, or the method of cutting in order to expose the brain or the eye, or the best mode of going to work with a view to properly examine the epithelium, or, finally, the practical teaching in reference to the physiological properties of muscle and nerve, in all these we recognise at once the practical skill as well as the scientific knowledge of a master-mind.

Throughout the other part of the book we observe the same thoroughly practised hand in the working of the several sections. This is what is so valuable to the student, as, for example, when there are given those hints as to the use of osmic acid solution in dealing with the Infusoria, the best mode of killing the hydra, the guarding the bristle with wax, the examination of the circulatory apparatus in the lobster, &c. &c., all of which point to the decidedly practical form of the work. We are glad to notice too that in speaking of magnifying powers he uses the definite expressions $\frac{1}{2}$ or $\frac{1}{4}$ or $\frac{1}{8}$ of an inch objective, and that there are only one or two instances in which the foreign method of designation, such as No. 7 or 8, is employed.

We are sure that English microscopists will be very grateful to Professor Huxley for having made a stand against the absurd method of nomenclature now adopted by many of our histological writers. We suppose it would have been impossible to introduce woodcuts into such a volume as the present one, although it would be of undoubted importance to the student who is ignorant of the different forms he is looking for. However, we may, in concluding this imperfect notice of a very important volume, express our thanks to the two gentlemen—Professor Huxley and Dr. Martin—who have laboured so well for our advantage.

CLIMBING PLANTS.*

ALTHOUGH this book is called a second edition it is really, so far as the general public is concerned, the first time that the work has been issued. It is true that it was first published in the "Journal of the Linnean Society," but then such publication merely supplies the book to the Fellows of that Society, whilst in its present form it is "comeatable" by the whole world. It certainly appears to us a wonder that Mr. Darwin should have kept such an important light under the bushel of a Society's publications, and that, too, for so long a period. However, now that he has given us the work in a clearer form, with additional facts, and with the few but clever engravings from drawings by his son, George Darwin, we have only to thank him, as we have always done, for the importance of his labours. The present book has an especial value over the earlier edition, in that it takes up the point which has been so ably discussed by Professor Sachs in his recently translated "Text-book of Botany," of the cause of the motion of tendrils. It is remarkable, too, that Mr. Darwin differs from Professor Sachs as to the cause of certain movements of the tendril, and in this it appears to us that there is much that is reasonable in Mr. Darwin's view. This, indeed, is the most interesting, because the most novel, part of the book. "Sachs," he says, "attributes all the movements of tendrils to rapid growth on the side opposite to that which becomes concave. These movements consist of revolving nutation, the bending to and from the light and in opposition to gravity, those caused by a touch and spiral contraction. It is rash to differ from so great an authority, but I cannot believe that one at least of these movements—curvature from a touch—is thus caused. In the first place, it may be remarked that the movement of nutation differs from that due to a touch, in so far that in some cases the two powers are acquired by the same tendril at different periods of growth; and the sensitive part of the tendril does not appear capable of nutation. One of my chief reasons for doubting whether the curvature from a touch is the result of growth is the extraordinary rapidity of the movement. I have seen the extremity of a tendril of *Passiflora gracilis* after being touched distinctly bent in 25 seconds, and often in 30 seconds; and so it is with the thicker

* "The Movements and Habits of Climbing Plants." By Charles Darwin, M.A., F.R.S., &c. 2nd edition, revised, with illustrations. London: John Murray. 1875.

tendril of *Sicyos*. It appears hardly credible that their outer surfaces could have actually grown in length, which implies a permanent modification of structure, in so short a time. The growth, moreover, on this view must be considerable, for if the touch has been at all rough the extremity is curled in two or three minutes into a spire of several turns." It must be at once confessed that in regard to this question the probability of the argument lies on Mr. Darwin's side. For it is utterly impossible to suppose that any development of tissue could occur in so short a period of time. But this is only one point of interest in the work before us, which contains many. For example, there is the curious fact that a climbing rose will ascend the walls of a house if covered with trellis, without there being any explanation of the fact. Mr. Darwin says :—"How this is effected I know not ; for the young shoots of one such rose when placed in a pot in a window bent irregularly towards the light during the day and from the light during the night, like the shoots of any common plant; so that it is not easy to understand how they could have got under a trellis close to the wall." And we do not see that Professor Asa Gray has done very much to help us on this point, though Mr. Darwin thinks he has. Indeed, we are very much disposed to consider that the tendency of certain climbing plants to creep along and to show an apparent knowledge of the parts on which they are growing is only to be explained by assuming the possession by plants of certain powers that have been hitherto allowed by Biologists to exist in animals alone. And we think that anyone who carefully reads this book will see that it is utterly impossible to explain certain motions of plants by the ordinary rules of botanists. We should have liked to quote the author's summary on this very important point, but as we cannot, we would especially direct our readers' attention to page 202, and that of succeeding paragraphs. We cannot do better than conclude our notice of this excellent book with the following quotation :—"We see how high in the scale of organisation a plant may rise when we look at one of the more perfect tendril-bearers. It first places its tendrils ready for action as a polypus places its tentacula. If the tendril be displaced it is acted on by the force of gravity and rights itself. It is acted on by the light and bends towards or from it, or disregards it, whichever may be most advantageous. During several days the tendrils or internodes, or both, spontaneously revolve with a steady motion. The tendril strikes some object, and quickly curls round and firmly grasps it. In the course of some hours it contracts into a spire dragging up the stem, and forming an excellent spring. All movements now cease. By growth the tissues soon become wonderfully strong and durable. The tendril has done its work, and has done it in an admirable manner."

THE DAWN OF LIFE.*

IN the above work Dr. Dawson has laid before us, in a clear and able manner, his views, and of those also who coincide with him, as to the nature of a remarkable fossil structure found in the ancient Laurentian

* "The Dawn of Life." By J. W. Dawson, LL.D., F.R.S. London, 1875.

rocks of Canada, the *Eozoon*, the origin of which has caused some difference of opinion, and produced a crop of descriptive and controversial, but for the most part technical literature, as to whether the structure was organic and foraminiferal, or the result of chemical and mineral metamorphism. The object of the book is therefore to give a popular account of what is known as to the organic structure of the Dawn-animal, stating the substance of all that has been previously published, and including many new facts illustrative of points hitherto more or less obscure. The matter is arranged under the following heads: a notice of the Laurentian formation itself; the history of the discovery and proper interpretation of the fossil; the description of the Eozoon and its mode of preservation, its contemporaries and immediate successors, or those allied to it by zoological affinity, and also the objections which have been urged against its organic nature. The lower and upper Laurentian rocks are 30,000 feet in thickness, and the Eozoon occurs in the lower; the upper series and the overlying Huronian being unfossiliferous, and together with the lower were termed the Azoic, from the supposed absence of all living things. Hence the interest in the discovery of the presumed dawn of life in the oldest of all the formations known to the geologist, and more thoroughly altered or metamorphosed by heat and heated moisture than any others. Under the head, What is Eozoon? Dr. Dawson enters into a full description of the structural characters of this form and their relation to other Foraminifera, and gives the corroborative opinions of other authors. "Its relation to modern animals of its type has been very clearly defined by Dr. Carpenter. In the structure of its proper wall and its fine parallel perforations it resembles the *Nummulites* and their allies; and the animal may be regarded as an aberrant member of the Nummuline group, which affords some of the largest and most widely distributed of the fossil Foraminifera."

The objections to the animal nature of Eozoon are fairly discussed, and of the three general modes for accounting for its existence, first that of Professors King and Rowney, who regard the chambers and canals filled with serpentine as arising from the erosion or partial dissolving away of the serpentine and its replacement by calcite; and secondly, that Eozoon forms are merely peculiar concretions; both of these are considered not conclusive. The third theory, that of filling of cavities by infiltration with serpentine, accords with the fact that such infiltration by minerals akin to serpentine occurs in fossils in later rocks, and thus explains to some extent the condition of Eozoon.

We may commend the book, with the excellent illustrations and descriptions, to the careful perusal of those who are interested in the character and real nature of the first presumed traces of organic life in the history of the earth, so as to understand the strong grounds upon which Dr. Dawson and the able naturalists who support his view regard the Eozoon as the oldest known Protozoon and related to the Nummulinidæ. On the other hand, the opponents of the organic character of the Eozoon have, from their point of view, given reasons for inferring that the nummuline-like structure may be the result of the aggregation and rearrangement of mineral matter due to chemical and metamorphic action; for it must not be forgotten, as the author states, that the appreciation of the evidence for such a fossil as

Eozoon requires an amount of knowledge of minerals, of the more humble types of animals, and of the condition of the mineralization of organic remains.

RELIQUIÆ AQUITANICÆ—CONCLUDING PART.*

IT was in the same month as that in which the present notice is written, and in the year 1865, that the first part of this splendid essay made its appearance. And now, in the last portion of the year 1875, the final "part" appears. In ten years how many mighty changes have taken place! The two great leaders of Geological Science have within that period gone to their last resting-place, and the two distinguished men—M. E. Lartet and Mr. Christy—whose names appear on the title-page of this work have likewise rendered their account to Heaven. The volume, therefore, although it is a very valuable one, is by no means the sort of work that Mr. Christy or M. Lartet would have made it had they lived. Indeed, if we have a fault to find, it is with the want of connection which exists between the different portions of the volume. It partakes more of the character of a journal to which different men contribute without the least regard to each other's writings, than anything else. It wants the connecting link which would surely have been presented had either of the original authors been in existence. How can it be otherwise when we have thrown together papers by Prof. Rupert Jones, M. Louis Lartet, Dr. Pruner Bey, M. de Quatrefages, Mr. J. Evans, Mr. A. C. Anderson, Mr. L. Austen, Dr. Sauvage, Dr. A. Milne Edwards, Dr. E. Harvey, and Mr. T. G. B. Lloyd, together with a few of the original authors' essays? The reader may at first sight consider that the editor is to blame for this. But in consideration of the statements he has put forward, we at once perceive that he is perfectly free from censure. He says the "mammalian remains are still undescribed, for M. Lartet's notes were left in too fragmentary and unfinished a state to yield a continuous memoir, and no other palæontologist as yet has turned his attention to the subject." However, even admitting this loss, we must confess that a tolerably good account of the people who lived in this period may be gleaned from the various descriptions given; and we may admit the editor's assertion that the features, stature, character, and race of the cave-dwellers in this portion of the globe have been fully detailed. "Their habits have been elucidated in the descriptions of their weapons and other implements adapted for shooting or darting, stabbing, clubbing, cutting, chopping, scraping, boring, drilling, and other work wanted in either peace or war; in hunting or fishing, in domestic operations; and in designing the works of art which so markedly characterised this peculiar people of Western Europe. Their cooking stoves, hearths, and mortars; their bodkins and sewing needles; their personal ornaments and amulets, perforated for stringing; their

* "Reliquiæ Aquitanicæ; being contributions to the Archæology and Palæontology of Perigord and the adjoining Provinces of Southern France." By E. Lartet and Henry Christy. Edited by T. R. Jones, F.R.S., F.G.S. London: Williams and Norgate. Nov. 1875.

whistling instruments and their batons, possibly distinctive of rank and dignity—have received much attention. . . . Even their owner-marks, tally-scores, and probable gambling tools have been recognised and described. How they made their many implements of flint, and why that stone was good for their purposes, has also been explained." Indeed, not an insignificant feature of the book is the contrast that has been made by some of the writers between the implements found in these French caverns and the various forms that are employed by several savages even of the present day. Two essays, which constitute the bulk of the present "part," are those of "Fossil Man from La Madelaine," and on "The Reindeer of Newfoundland." They are exceedingly well illustrated, and though short are to the point. As to the engravings, we have almost always spoken highly of their execution. Those in the present part are merely woodcuts, but all of the plates, except one or two that were executed in England, are simply admirable in all respects. In conclusion, we must not forget that a word is due to the executors, who have spared no expense in bringing their brothers' intended design to completion. We think that the public owe them sincere thanks for their labours, and in selecting Professor Rupert Jones as editor they could not have better supplied a guiding hand.

RELIGION AND SCIENCE.*

HERE we have a book written by a Rector upon the reconciliation of Science and Religion, and at the outset we must confess that it is written in so fair and honest a spirit that it seems to us more detrimental than conservative to religious views. The author has attempted to reconcile the present condition of science to his own exalted type of Christianity, and in doing so he has not used a syllable that can in the slightest way offend the man of science. Indeed, he has gone so far in his admissions that it strikes us very forcibly that to a real student of science it is a book which would go far to alter his Christianity, if it already existed. Mr. Gibson has made himself acquainted with all that men of the type of Mr. Darwin, Professor Huxley, Mr. Mill, Sir W. Thomson, Sir J. Lubbock, Sir W. Hamilton, Professor Max Müller, Sir Charles Lyell,—not to speak of Paley and many other older authorities,—have written; and he himself comes down from the standard that would be alone accepted by most conservative churchmen, and in giving up Paley's doctrine of adaptation to special ends, and acknowledging Mr. Darwin's view of Natural Selection, he endeavours to show that he is right in the assumption that if a Deity in the ordinary sense of the word does not exist, there is nevertheless such a being as is immensely superior to man's organisation and who will not look on him in judgment in a Pharisaic spirit, but will prove to be considerate and kind. His argument is, it seems to us, wordy and diffuse, and while it will certainly

* "Religion and Science, their relation to each other at the present day." Three Essays on the grounds of religious belief, by Stanley T. Gibson, B.D., Rector of Sandon, in Essex. London: Longmans. 1875.

not satisfy the unbelieving man of science it will very probably give the "first shake" to the opinions of those who have gone through life with their religious faith in one hand and their scientific belief in the other. But while we object to the space over which Mr. Gibson has taken us, we confess that we have been enlightened on many points in our journey together, and we thank him heartily for the extreme courtesy he has invariably exhibited. To show that the author is one who has clearly a scientific mind the reader need only refer to the following passage in which he alludes to a not infrequent habit even among scientific men:—

"They talk sometimes of tracing things back to their nebulæ, as though they were thus obtaining a comprehensive view of the entire history of creation. But supposing that the solar system did originate from a nebula, have they a right to assume this development to be coeval with the universe? We have good reason to think that stars are by no means of equal antiquity, and that our own sun is not one of the oldest. Further, that nebula out of which it sprung need not be thought the primitive state of matter. It might have been the result of some previous condition of things, say a collision of two great cosmical bodies and the vaporising of their masses."

There is another paragraph we should like to have inserted, as it shows the broad views of the author. It is that in which he takes to task Professor Birks for his assertion in the Scripture doctrine of creation that "if there never was a beginning of time the present moment could never have arrived." There are many points in the argument which the present writer would wish to dwell on, but that space forbids. He therefore bids, he hopes, *au revoir* and not *adieu* to Mr. Gibson, and thinks he has with the worst materials at command made out a most excellent brief in the present case.

POPULAR OPTICS.*

WE are sorry to be compelled once again to find fault with one of Messrs. King's handbooks, the more so as we feel assured that that eminent firm has nothing whatever to do with the work under notice further than their labours as publishers, which are performed in a most thoroughly satisfactory manner. In its mere mechanical details the work is capitally done: no fault can be found with either the printing or illustrations of the book before us. But as a popular essay on a most interesting branch of optics, it is the most incomplete work that we have for a long time met with. The author seems to be devoid of that power of popularising his subject which is possessed in so marvellous a manner by our own Tyndall. And he has added to the difficulty by endeavouring to introduce elementary mathematics. Further, his descriptions are extremely incomplete in nearly every instance. Take, for example, what he has to say on the microscope as

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* "The Nature of Light, with a general account of Physical Optics." By Dr. E. Lommel, Professor of Physics in the University at Erlangen. With 188 illustrations, &c. Henry S. King & Co. 1875.

an illustration. In this he introduces us in about a page and a half to one of the foreign instruments, and he absolutely dismisses the subject without any allusion to the two lenses of the eye-piece, or even in the most rudimentary shape or form to errors of chromatic or spherical abberration, or to the fact that there is such a condition as that of immersion in object-glasses. His observations on the subject of the spectroscope are wrapped in the same Cimmerian darkness. And his idea that the spectroscope may "render important service to physiology and forensic medicine," by the fact that poisoning by hydrocyanic acid may be immediately recognised by it, is of course absurd, because the blood would never be examined until all trace of the poison had passed away. We have no doubt that the author is a thoroughly competent authority, but we are equally certain that he is not a populariser of science, and we therefore regret that he has been employed by Messrs. King, to some of whose books of this series we have given the very highest praise. We shall be much surprised indeed if it ever achieves popularity among our English readers.

ZOOLOGY FOR JUNIORS.*

WE fancy that we spoke well of this book when it appeared in its first edition. If so our praise was given where it was due, as the passage of the work through a second issue pretty conclusively demonstrates. It is a book well and clearly written and very well illustrated—more than 150 woodcuts—and the author has very properly placed the name of the writer from whose works he has taken the illustrations after all woodcuts, except those which have been in service for the past thirty years. Thus we notice cuts from Huxley, Forbes, Johnston, Gosse, Hincks, Ehrenberg, Schmidt, Müller, Carpenter, Carter, Beale, Günther, Dümeril, Van der Hœven, Woodward and others, showing that he has been careful to select from good authorities; and the matter of the book is fairly compiled and many additions have been made to the Vertebrata, so that we can congratulate the author on the success of his book, which of course is an elementary one.

TAPE-WORMS.†

THIS is a book to which is hardly due a place in this Review, for it really is a medical work. Still, it treats upon a subject of which the author is the first scientific exponent in this country, and therefore it

* "An Introductory Text-book of Zoology for the use of Junior Classes." By H. Alleyne Nicholson, M.D., D.S.c., M.A., Professor of Natural History in the University of St. Andrews. 2nd edition. Blackwood and Sons: Edinburgh, 1875.

† "Tapé-worms. Their Sources, Varieties, and Treatment; with one hundred cases." By T. Spencer Cobbold, M.D., F.R.S. 3rd edition. London: Longmans. 1875.

may partly lay claim to a word or two. Dr. Cobbold has dealt with the subject of tape-worms and their treatment, and he has nearly 100 cases in which he records almost invariable success. In most of these cases the common male fern was the remedy successfully employed, and we think that if the prescriber is cautious in obtaining a proper sample of his drug, his results with this medicine are likely to be good. However, there are some instances in which the worm cannot be removed. The writer of the present notice remembers two cases which were under his own treatment—one for more than three months—and which he was compelled to send away uncured. They had both been in India, had been under the care of military medical men and "club doctors," and one of them had spent some weeks at Netley Hospital,—and yet they were not cured. This little book gives sensible advice to the medical man—advice not always, we are sorry to say, followed,—and we are glad to see it in its third edition.

SCIENCE BY-WAYS.*

In this volume Mr. Proctor has expressed his wrath with certain critics, more especially those of the American school, and we think there is much justice in his remarks. Mr. Proctor has been blamed for writing popular treatises, and we cannot at all see for what reason people who are not his immediate friends can object. There is not the slightest doubt that he wields a clever pen, and in addition to this he comes before the public with exact knowledge and with a power of popularising a subject which very few possess under such circumstances; then who can object to his writing as many works as he pleases to issue? We certainly do not see what right critics have to cavil. They have to review a book like that before us at this moment, which possesses intrinsic worth, which popularises in a telling manner, and yet without any slovenliness as regards its scientific value. Why, then, can they not do their work without attacking the author? We cannot tell, unless it be that private pique prevents them. For ourselves we only regret that one who possesses the unquestioned ability of Mr. Proctor should expend his time in popular essay-writing rather than in noting down the observations made with the telescope or the spectroscope. But we hail with pleasure the announcement of the issue of a new work from his pen. In the present book we have a reprint of a series of essays, most of them astronomical, but one or two physiological, and indeed to our mind these are the most interesting of the whole assemblage. It deals with "life past and future in other worlds," the planets put in Leverrier's balance, comets' tails, three orders of comets, the sun a bubble, the sun's surroundings and future eclipses, the weather and the sun, finding the way at sea, journeys towards the North Pole, rain, damage from lightning, growth and decay of mind, have we two brains? on some strange mental feats, automatic chess

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* "Science By-ways, &c. &c.; to which is appended an Essay entitled 'Money for Science.'" By R. A. Proctor, B.A., &c. London: Smith & Elder. 1875.

and card playing, and last, but by no means least in importance, money for science, a brilliant essay which appeared, if we mistake not, in the "Contemporary Review." All of these several chapters are of interest, but that on "Have we two Brains?" strikes us as of especial importance. We remember reading the report of Brown-Sequard's lecture on the subject, which has formed the basis of Mr. Proctor's chapter; but it was so exceedingly badly reported that we could make nothing of it as a physiological argument. We have read Mr. Proctor's pages on this subject with great interest, therefore; and while we differ from him on some points we agree with him on others; and we would especially commend his mode of dealing with the question to our readers. All through his book is of more than general interest and value.

POPULAR CONCHOLOGY.*

ONE would have thought that the ordinary British Mollusca of our ponds, streams, and groves had been described enough. However, there is, we suppose, still room for a good cheap popular description, with fairly coloured plates; and such a book is that before us, by Mr. J. E. Harting. The present work is an enlarged reprint from the "Field" newspaper, and it deals with the subject in such a manner that any person of ordinary intelligence can identify the species described. The author has adopted the plan of grouping the shells according to the soils which they inhabit, thus giving the reader an opportunity of finding a number of specimens on each excursion. We think the idea is good for a popular book of the kind. The volume has had the advantage of the revision of the late Dr. J. E. Gray, F.R.S., and it is one we can commend.

WHAT IS AIR? †

ONE who did not understand chemistry would hardly comprehend how it was that a whole book was required to answer the question in the above heading. Yet so it is, and Mr. W. N. Hartley has given us a very excellent and scientific, yet popular account of air in the 243 pages which form this volume. It is, in fact, but a species of report of his lectures, which were last year (1874) delivered before the Royal Institution, on the subject of Air in its relation to Life. However, the author has doubtless expanded many of the points he referred to in his discourses,

* "Rambles in Search of Shells, Land and Fresh Water." By James E. Harting, F.L.S., F.Z.S. With coloured illustrations. Van Voorst. 1875.

† "Air, and its relation to Life; being, with some additions, the substance of a Course of Lectures delivered at the Royal Institution in the year 1874, by W. N. Hartley, F.C.S." London: Longmans. 1875.

and he has added a number of illustrations—more than 60—which give more interest to the volume. The greater part of the volume is familiar enough, but the latter part is of peculiar interest, because it contains an ample and clear account of M. Pasteur's experiments on air and fungus-spores, &c.; and it shows the error of Dr. Bastian's processes, though in some cases there is rather too much of opposition displayed to leave the reader perfectly satisfied on this point. The concluding observations which the author makes on the subjects of putrefaction and decay are full of interest, and might we think with advantage have been much extended. The truth is shown of Pasteur's saying, that "Fermentation, putrefaction, and slow combustion are the three natural phenomena which concur in the grand operation of the destruction of organised matter, a necessary condition of the perpetuity of life on the earth's surface."

THE UNIVERSE.*

IN speaking of this splendid work as it appeared in its first edition we gave the highest praise to the author for the ability displayed in carrying out his scheme, and to the publishers for the lavishness of illustrations with which they presented the volume to the public. In noticing the present edition—the third—we have little more to do than to say that it is issued at a much cheaper rate of sale than its predecessors, while it has not suffered any serious loss by the omission of a few of the illustrations and of the notes which characterised the earlier issue. It is almost needless to tell the English reader that M. Pouchet—the great supporter of the doctrine of Spontaneous Generation in France—was one of the first naturalists of the day, and he has therefore given us a book which is an unquestionably admirable introduction to the study of natural history as a whole. As he says in the preface, "I have gleaned everywhere to show that Nature affords matter for interesting observations. The animal and the vegetable worlds, the earth and the heavens, appear by turns upon the scene." Indeed, there could be no better description of the work. It treats of Nature as a whole, and in the most fascinating style—well sustained in the translation—the author has told us the history of the animal and vegetable worlds in the present and in the past, and has added some observations on the sidereal or starry system, which, though small in their extent, are nevertheless of importance. Truly, too, the artistic portion of the work has been placed in the most reliable hands.

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* "The Universe; or, the Infinitely Great and the Infinitely Little." By F. H. Pouchet, M.D., Member of the Institute of France. Third Edition. London: Blackie & Co. London, 1876.

ASTRONOMY FOR THE PEOPLE.*

MUCH as is the custom adopted of crying down the once popular books of the late Dionysius Lardner, there is no doubt that they have not yet been succeeded in many instances by books as well adapted to the mind of the people. And indeed we know of few cases in which books on Astronomical Science appeal so truly to the public, as in the present instance. Mr. Edward Dunkin, Sec. R.A.S., has been the editor selected for the present volume of Dr. Lardner's series of books on Natural Philosophy, and we can say—as of course was to be expected—that he has done his work with conscientiousness and clearness. The present edition was published early in October last, and Mr. Dunkin has added to Chapter XV. planets 147 and 148, the last of which—which, by the way, is not now the last—was discovered Aug. 7, 1875. One of the features of the present edition is that the elements of the orbits of all the planets are given, being arranged in the order of the distances of the planets from the sun. It is elaborately illustrated; besides more than 100 woodcuts, there are thirty-seven admirably executed plates of the different heavenly bodies, telescopes, &c. Besides this is a copy of Beer and Mädler's map of the moon, and, we are almost pleased to say, no spectroscopic plates at all. It will be found a book worthy of being read by the class for whom it is intended.

SHORT NOTICES.

Lecture on the Geology of Croydon, in relation to the Geology of the London Basin and other localities. By J. Morris, F.G.S. The "Chronicle" Office, Croydon. 1875.—This is the publication of a lecture delivered by Professor Morris before the Croydon Microscopical Club, and though it has so humble an origin, it is not on that account to be classed with literature of this order. It is really a most valuable addition to our geological history of the London Basin, and we trust that the Professor had a number of copies struck off, for we are sure that the demand for the work will be considerable when it is known of. There are very few, if any, of our English geologists whose knowledge of the strata, both geologically and palæontologically, equals that of the author of this lecture; and he has given us in a pamphlet of about 27 pages, accompanied by an admirable coloured map and section, and several woodcuts of the district, a discourse in which he discusses the origin of those various beds and their fossils, and deals with the several elements of the London Basin. It would be impossible to follow the author in a sketch like this; but we must mention that he traces out the whole series of deposits from the chalk upwards, and he adds considerably to the interest of his paper by his marking out the beds which occur in Paris and Belgium, and are consi-

* "Handbook of Astronomy." By D. Lardner, LL.D. Fourth Edition. By Edwin Dunkin, Secretary to the Royal Astronomical Society. London: Lockwood & Co., 1875.

dered to be the representatives of our London deposits. By far the most interesting parts of the paper are the notes, which we presume were afterwards added. For in these, which are nearly as extensive as the text, the author gives us the opinions of Dr. Hayden and Professors Lesquereux and Cope in reference to the singular want of nonconformity which exists between the cretaceous and succeeding strata, and alludes to the fact that, in reference to the chalk and the tertiary flora, a complete "uninterrupted succession of life" took place. In his remarks, too, on the London clay, we find an abundant reference to the numerous and diversified fossils, animals and plants, of the period, the immense number of turtles (no less than ten species being represented in the clay of Sheppy) giving the Professor the opportunity of a joke *à propos* of our civic authorities. One of the most important points of the lecture is the table at the end, which shows us at a glance the formations from the cretaceous to the recent alluvium, which are present or absent from the London Basin (at Croydon, London, and Hants) and the Belgian and French corresponding deposits. The London geologist should be grateful to Professor Morris for the task he has undertaken.

Time and Time-tellers. By James W. Benson. London: R. Hardwicke. 1875.—This little book, although it is doubtless issued with the object of being a trade publication, is nevertheless a very interesting work for those who care for a chatty, gossiping account of the history of clocks and watches from the earliest device down to the period of Benson's great clock which was exhibited at the International Exhibition of 1862. The history is well written and fairly stated, and the explanation is given of the several parts of a watch or clock which have been from time to time added as improvements. Finally, there is a useful table at the end of the work of the equation of time, by means of which, with an ordinary sundial, one may readily calculate the exact time for himself.

SCIENTIFIC SUMMARY.

[*The Editor feels that he must apologise to his readers for the extreme shortness of the various summaries in the present Number. The explanation is, that room had to be made for the articles which have—in one case especially—far outrun their usual and proper limits.*]

ASTRONOMY.

THE *Sun.*—According to the Astronomer Royal, the spots at present are fewer than he has ever known. Photography is being applied to them at Greenwich. A valuable series of photographs, comprising more than an entire spot-cycle of eleven years, has been presented to the Astronomical Society by the executors of Professor Selwyn. Padre Secchi has reported his observations from April 23 to June 28 to the Académie des Sciences. He had been recording, not, as formerly, the number, but the area of the spots, which were steadily diminishing, as well as the daily number of protuberances. The more vigorous eruptions ceased as the larger spots disappeared; lofty protuberances had become very rare; those at the pole had greatly decreased; and the faculæ which had formed polar coronæ had vanished; so that on the whole we may be passing through a *minimum* of these phenomena.

The Moon.—The remarkable flattenings of the limb to which the Rev. H. C. Key drew attention twelve years ago were re-observed on Nov. 11 or 12, to great advantage, by himself, Messrs. Birt, With, and Erck. Mr. Key says that if these two depressions had been repeated all round the limb its form would have been a very decided dodecahedron. The suitable libration had been previously computed by Mr. Marth, but as Mr. With had seen them even more strikingly during the previous lunation, it is evident that they are worth looking for at other epochs. Their interest arises from the fact that they cannot be the profile exhibitions of great plains, which would still preserve the general convexity of outline, but must have the effect of concavities as respects the centre of the moon; resembling nothing, at least of any magnitude, in the visible hemisphere: an indication, perhaps, that the formations at the back of the moon may not be entirely similar to those on this side.

Mars.—Dr. Terby, of Louvain, is actively occupied in collecting observations and drawings of this planet. The Observatory at Leyden has purchased the "Areographische Fragmente" in two MS. volumes, left unpublished by Schröter. Kononewitsch suspects a diminution of the planet's brightness. The Astronomer Royal has pointed out the importance of a renewed investigation of the solar parallax from the opposition of Mars in 1877, which he considers preferable to the transits of Venus, and will publish in the Monthly Notices a chart of the stars to be observed with the planet at that epoch.

The Minor Planets.—Discoveries in this region have been increasing in an unprecedented ratio. No. 150 was detected, Oct. 10, by Watson at Ann Arbor.—No. 151 by Palisa at Pola (on the Adriatic), Nov. 1.—No. 152 by Paul Henry at Paris, Nov. 2.—No. 153 by Palisa, Nov. 12.—No. 154 by Prosper Henry at Paris, Nov. 6.—No. 155 by Palisa, Nov. 8.—No. 156 by Palisa, Nov. 22. (These numbers are not in order of priority, and probably will have to be rectified.) So rapid a development of our knowledge in this direction, amounting to sixteen during the present year, and six in one month, is likely to cause embarrassment. Computation already begins to be uncertain, and failures frequent in rediscovery.

Jupiter.—M. Flammarion, who has been drawing this planet, has noticed various remarkable changes in the colour of the luminous zones, and a number of white elliptic spots, seemingly followed by ill-defined shadows, and terminating in angular trains, as if the shadow passed through separate strata of clouds.—Miss Hirst, of Auckland, New Zealand, has sent to the Royal Astronomical Society some drawings made with an $8\frac{1}{2}$-inch silvered glass Browning reflector during the opposition of 1875. On one occasion a number of small dark spots with extremely black centres were seen on the S. polar zone; at another time a small oval patch of a decided sea-green for three days near the same pole. Lassell's bright spots were twice observed. It is evident from these and other observations that we are at present far from having attained any consistent interpretation of the phenomena of this colossal planet; and the want of consent among contemporary drawings points to the conclusion that something better must be accomplished in this way before we can make satisfactory progress. The inscription must be more faithfully copied before we can attempt to read it; and probably this will not be done excepting in the use of those commanding instruments, now fortunately becoming more common, in which the size and brightness of the picture will diminish the ratio of personal equation to a much more unimportant fraction than that which now represents it. Dr. Terby may, we hope, be induced to repeat on this most interesting planet the investigation which he has so ably and diligently conducted in the case of Mars. —Le Verrier has investigated the mass afresh, and adopts $\frac{1}{1046\cdot77}$, the value given by Airy from observations of the fourth satellite in 1835. This would of course be preferable to the old result, $\frac{1}{1067\cdot09}$, obtained by Laplace from Pound's observations; but he finds it superior also to Bouvard's (1824) of $\frac{1}{1070\cdot5}$, deduced from the perturbations of Saturn by a method which he discovers to be inexact, but which accidentally led to nearly the same value as Pound's. From Triesnecker's observations in 1794 and 1795, Bessel had brought out $\frac{1}{1055\cdot68}$; Nicolai, from fifteen oppositions of Juno, $\frac{1}{1053\cdot92}$; Encke, from fourteen oppositions of Vesta, $\frac{1}{1050\cdot36}$; Santini had deduced $\frac{1}{1049\cdot2}$; Bessel, from his own measures, $\frac{1}{1047\cdot879}$; Krüger, from the perturbations of Themis,

$\frac{1}{1047 \cdot 16}$; Möller, from Faye's comet, $\frac{1}{1047 \cdot 79}$; Von Asten, from that of Encke, $\frac{1}{1047 \cdot 61}$.

Saturn has been very unfavourably situated for English observers; but the position of the satellites has been compared with Marth's ephemerides by Mr. Christie at Greenwich.—Le Verrier has compared his theory of this planet with the observations of the last thirty-two years, and finds the result satisfactory, some slight discrepancies being probably due to the varying aspect of the ring.

Uranus.—Professor Newcomb, who is in charge of the great achromatic at the Washington Observatory, of 26 inches' aperture, has made a special study of the satellites of this body during the early part of this year. He fully confirms Lassell's opinion that there are only four, the orbits of which he finds nearly circular and in the same plane. The brighter satellites, *Oberon* and *Titania*, appeared in this noble instrument about equal to fourth magnitude stars with the naked eye; the two inner ones he thinks the most difficult of well-known objects, but was surprised at the precision with which he could bisect them. They were pretty certainly discovered by Lassell, and have not been, he thinks, subsequently seen by any one except himself; they are claimed, however, by the Melbourne reflector. Sir W. Herschel's outer satellites he pronounces non-existent. The feeble perturbation of these minute bodies by each other or by the far-distant sun in the immediate presence of their overpowering primary enables the mass of the latter to be ascertained with considerable exactness, and the Professor deduced a value of $\frac{1}{22600}$. The accurate focusing of the eye-piece was, however, disturbed by the differing colour of the redly illuminated micrometer webs and the greenish-yellow satellites, and this may somewhat affect the result. No markings were detected on the disc. It is believed that the two brighter satellites are within the grasp of the larger telescopes in England.

Observatories and Instruments.—The Paris Observatory is now opened by Le Verrier's orders three times a week in the evening, weather permitting, and two large telescopes are placed at the disposal of visitors provided with letters of admission, obtained by application to the secretary. This is a move of the most commendable nature, and an example which we should do well to follow. The arrangement of our public observatories might not admit, generally speaking, of such an interruption; but it might be worthy of consideration whether observatories for popular and educational purposes might not be established in our larger cities, provided with sufficient instruments and attendants, the expenses of which might be met by subscriptions and entrance charges.—We regret to find that Mr. Hind's observatory at Twickenham, the 7-inch Dollond achromatic in which did such excellent service at Mr. Bishop's in Regent's Park, is to be dismantled, and the instruments presented to the Royal Observatory at Naples.—Winnecke at Strasburg describes a new orbit-sweeper, and announces the commencement of a review of nebulæ.—The observatory of the Rev. H. C. Key, at Stretton, near Hereford, is now in a high state of efficiency, being provided with two 18-inch silvered glass specula, perfect to the edge, one of which is equatorially mounted and driven by clockwork. There is a filar micrometer and apparatus for wire-illumination by galvanic current, and a large and ponderous spectroscope by Browning, with two prisms, of which one only can be used for very faint objects, the collimator lens and object-glass of inspecting telescope having each an aperture of 1½-inch; there is a tangent-screw micrometer, and filar ditto arranged to throw an illuminated image of the webs across a faint spectrum; besides the usual appliances of transit, clocks, &c.—Mr. Lockyer and Major Festing have been sent by the English Government to Rome, with the object of borrowing from the Italian Government a collection of interesting astronomical instruments, to be exhibited next year at South Kensington.—Mr. Lick, it is said, has selected Mount Hamilton, in Santa Clara county, California, 4,448 feet high, with no rival within fifty miles, as the site for the "million dollar telescope."—The largest achromatic ever made will shortly be in progress at Mr. Grubb's new factory near Dublin. The aperture will be 26 inches, or 27 if the discs, which are the production of M. Feil, of Paris, will admit of it. It is intended for the grand new observatory at Vienna, and will probably not be finished before 1878.—A 12¼-inch object-glass by Grubb has recently been mounted at the new Observatory at Oxford.—Feil's glass, which seems likely to supersede that of English manufacture, not now as successful as in past years, has been already tried and approved by Wray.—Alvan Clark, it is said, has received an order from the Austrian Government for an immense reflector, to be placed in a new observatory at Trieste. (For "reflector" we should probably read "achromatic," though we have heard that specula occupied the earliest attention of that great optician.)—The new great reflector at the Paris Observatory is said to turn out very satisfactorily. The movable part weighs nine tons, the speculum of silvered glass half a ton, its diameter being 120 centimetres (47¼ inches), and focal length 6·8 metres (22 ft. 4 in.) The front view has been adopted. It is said to give good definition of minute stars; though we may be pardoned for suggesting that if it was finished by the method of retouching devised by Léon Foucault, its figure is hardly likely to equal those worked by the direct process adopted by the most eminent makers in England. This magnificent instrument, which is protected, when not in use, by a gigantic iron cover, movable on rails, has been six years in construction, at an expense of 8,000*l.*, of which one quarter was absorbed in the speculum alone. Our neighbours, in the midst of their political difficulties and social distresses, have shown a noble example of scientific munificence, which we earnestly desire to see followed among ourselves, and we cordially wish the new telescope success.

BOTANY AND VEGETABLE PHYSIOLOGY.

Mr. Cooke's "Mycographia."—The first part of this splendid work, which is accompanied with coloured illustrations, has just made its appearance. It contains descriptions of *Geoglossum* and *Peziza*. It is published privately, and we are informed that only a very limited number of copies will be issued. Its title is "Mycographia seu Icones fungorum." It is not stated at what time it will be concluded, but it will not be issued oftener than once in every six months. It is a work which every lover of fungi should possess.

The Atlas of Diatomaceæ.—Several parts of this excellent work have now been published, and Mr. Kitton, one of our best authorities on the subject, gives this work very high praise indeed in a notice published in "Grevillia." Certainly the plates, though a trifle rough, appear excellently executed, and they contain enormous magnifications of the objects illustrated. The number of illustrations is numerous in each part.

Influence of Nutrition on Form.—At a recent meeting of the Academy of Natural Sciences of Philadelphia, Mr. T. Meehan remarked that the influence which nutrition, in its various phases, had on the forms and characters of plants was an interesting study; and in this connection he had placed on record in the Proceedings of the Academy, that two species of *Euphorbia*, usually prostrate, assumed an erect growth when their nutrition was interfered with by an *Æcidium*—a small fungoid parasite. He had now to offer a similar fact in connection with the common Purslane (*Portulaca oleracea*), one of the most prostrate of all procumbent plants, which, under similar circumstances, also became erect.

Some new Plants from the Nicobar and Andaman Islands.—Herr S. Kurtz has a very interesting paper on this subject in the "Journal of Botany" for November 1875, from which, however, we only abstract some of the physical facts recorded. The most remarkable one is the nature of the clay. Herr Kurtz says that the interest which attaches to the Nicobar vegetation rests chiefly in the peculiar polycistine clay, which looks somewhat like meerschaum, and is also nearly as light and porous. This clay covers large areas on those islands which form the so-called northern group. It contains, according to Dr. Rink's analysis —

Silica	72·2
Oxide of iron	8·3
Alumina	12·3
Magnesia	2·1
Water	5·6
	100·5

Here the total absence of alkalies is very remarkable. In places it becomes red from abundance of oxide of iron, and in this case it is usually literally filled with fossil seaweeds. A microscopical examination of the rock reveals abundance of silica, fragments of polycistines, and diatoms. One would say that on such substrata nothing but wretched scrub and harsh grasses could vegetate; but an examination of the greater part of Kamorta has taught me that luxuriant tropical forests, with an average height of about 80 ft., not only cover the seaside, but the same forests form belts of considerable breadth over the island itself, while the inner hill plateau is covered by those peculiar park-like grasslands which Dr. Diedrichsen has called grass-heaths. The next rocks botanically influential are calcareous sea sand, raised coral banks, limestone and calcareous sandstones, which belong to the so-called southern group, in which, however, Katchall (an entirely calcareous island) is enumerated. Then come the plutonic rocks and their *detritus*, which, however, were only little developed in those parts which I visited. All islands consisting of the above rocks are characterised by the absence of grass-heaths, and are covered with forests from the bottom to the top. The four principal aspects of vegetation in these islands are—1, mangrove swamps; 2, beach forests; 3, tropical forests, which fall under three groups, those growing on polycistine clay, those on calcareous or coralline strata, and those growing on plutonic formations; 4, grass-heaths.

CHEMISTRY.

The Composition of the Water of the Nile.—Mr. J. A. Wanklyn has published an interesting note in the "Chemical News" (Oct. 29, 1875) on the subject of Nile water and its varying composition. Of course our readers are aware that the Nile rises steadily to a vast height, reaching its highest level about the middle of September, and being lowest at about Christmas. The cause of the rise of the river is said to be heavy rains in the months of April and May, the effect of this rainfall requiring the lapse of a considerable time in order to exert its full influence. Possibly, too, the melting of snow on mountains near the sources of the river may concur in flooding the river. The height to which the Nile rises, as well as the exact period of the rise, varies from year to year, but it may be stated broadly that from the end of May until Christmas the Nile is more or less in flood, and from Christmas to the end of May the Nile is low. The following is a tabular statement of the composition of the water in the different months :—

Water of the Nile.

Date of Sample.			Grains per Gallon.		
			Solids.	Chlorine.	Hardness.
1874	June	8	15·0	1·80	7·0
	July	19	13·0	0·90	6·0
	Aug.	12	12·0	0·30	8·5
	Sept.	20	10·0	0·40	8·0
	Oct.	12	11·0	0·40	7·5
	Nov.	12	12·0	0·50	8·0
	Dec.	12	9·0	0·45	6·5
1875	April		16·0	1·00	8·0
	May	13	22·0	1·20	10·0

The remarkable point brought out in this table is the great relative alteration in the proportion of chlorine, that whereas in the beginning of June, just at the beginning of the rise of the Nile, the chlorine amounts to 1·8 grain per gallon the chlorine sinks to 0·3 grain per gallon when the Nile has attained a great size, and remains at very little above that proportion until the end of the year.

The Quantity of Tannin in Tea.—This question has been, with many others in relation to the chemistry of this plant, gone into by Mr. T. Wigner, whose papers have been published in the "Chemical News." The number of the "C. N." for Nov. 12, 1875, contains the author's remarks on the tannin question. He says that the percentage of tannin in tea is very variable, and there is little doubt that this is in a great degree the cause of the erroneous estimate which English tea-drinkers frequently make of the dietetic value of tea. They prefer tea which gives a dark-coloured infusion, and has some sensible astringency, to those varieties which give a paler and less bitter liquor. This probably accounts, to some extent, for the high estimation in which some kinds of Assam are held for mixing purposes. The acetate of lead process seems more reliable for determining the percentage of tannin than the old gelatine process, and it is certainly easier. I have therefore adopted it. I find that a sample taken from a mixture of six samples of Assam tea gave 45·5 per cent. of tannin, while some of the highest results were—

		Per Cent.
No. 82.	Moyune young Hyson	30·0
No. 83.	Very choice Assam	33·0
No. 1.	Indian young Hyson	39·0
No. 97.	Assam tea from Dr. McNamara's garden	27·7
No. 75.	Caper (mixed)	42·3

GEOLOGY AND PALÆONTOLOGY.

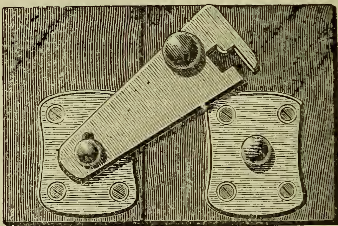
A New Crustacean Fossil from America.—This fossil, which was exhibited by Messrs. Grote and Pitt, at a recent meeting of the Buffalo Society of Natural Science, is of much interest. The specimen shown exhibited an impression of the ventral surface of a new form of crustacean allied to *Eurypterus Pterygotus*, for which the name *Eusarcus scorpionis* is proposed. The cephalothoracic portion appears to be separate from the body, and to be considerably narrower in proportion than in allied forms. The legs are the same in number as in *Eurypterus.* The swimming feet appear to differ by the straighter, less rounded outer margins. In the specimen the rhomboidal plates are not given. From the impressions of the joints of the swimming feet their relative dimension does not seem to accord with *Eurypterus.* The four pair of anterior feet proceed from two elongate oral plates, of which the impression is very distinct. The spines of the anterior feet appear to be long curved, and to have an anterior direction. The absence of chelate appendages to the posterior margin of the feet is particularly noticeable. The first seven broad segments of the abdomen form a large ellipse. There is an evident and remarkable narrowing of the succeeding caudal segments. Of these six appear to be made out on the specimen. The surface of the cast is punctate with scattered triangular impressions. The cast shows a widening of the terminal segment, and no traces of a spiniform process are exhibited. This portion of the fossil was imbedded in the matrix, and seems to have been bent downward and outward in the specimen. The greater flexibility of the caudal segments may be inferred from their shape and position. The specimen from which the above description is drawn was found in the Water-lime group at Buffalo, N. Y., in the same bed which has furnished specimens of *Eurypterus lacustris.* The length of the entire specimen is 250 millimeters; the greatest width of the body is 110 millimeters.

Lignite found in the Cretaceous Formation of the United States of America.—Mr. N. H. Winchell, in a letter to Mr. Dana, published in "Silliman's American Journal" (October 1875), says that an interesting discovery of lignite in the extreme northern portion of Minnesota has just been made. Explorations have been carried on during the summer by private parties in search of coal, and a hundred pounds of what was taken for coal were brought to Duluth from a point between Vermilion and Rainy lakes. Similar exploration has revealed the lignites of the Cretaceous at a number of other points in Minnesota, extending from near the Iowa State line to the central portion of the State. Mr. Kloos has given an account of a "Cretaceous basin in the Sauk Valley" in "Silliman's American Journal," and Mr. Meek identifies it as the Fort Benton by the few fossils that were gathered. Inferentially the Cretaceous has been extended over the most of Minnesota, and even into the State of Michigan ("First Annual Report of the Geological Survey of Minnesota, 1872"), but this discovery not only shows that the Sauk Valley is probably not an insolated "basin," but also that the Cretaceous beds did extend, prior to the drift, at least, if they do not now, over the entire State from north to south. He has an account also of a probable Cretaceous outcrop in the state of Winconsin from Prof. Frank H. Bradley, but it has not been identified authentically.

The Climate of the Poles, Past and Present, may not seem a very geological subject, yet it is one of the most interesting in the whole range of geological studies. A very valuable paper on this question has been contributed to the "Geological Magazine" (Nov. 1875), by Prof. Nordenskiold, in which he says that we now possess fossil remains from the polar regions belonging to almost all the periods into which the geologist has divided the history of the earth. The Silurian fossils which McClintock brought home from the American Polar Archipelago, and the German naturalists from Novaja Semlja, as also some probably Devonian remains of fish found by the Swedish Expeditions on the coasts of Spitzbergen, are, however, too few in number, and belong to forms too far removed from those now living, to furnish any sure information relative to the climate in which they have lived. Immediately after the termination of the Devonian age, an extensive continent seems to have been formed in the polar basin north of Europe, and we still find in Beeren Island and Spitzbergen vast strata of slate, sandstone, and coal, belonging to that period, in which are imbedded abundant remains of a luxuriant vegetation, which, as well as several of the fossil plant-remains brought from the polar regions by the Swedish Expeditions, have been examined and described by Prof. Heer of Zürich. We here certainly meet with forms, vast *Sigillaria, Calamites,* and species of *Lepidodendra,* &c., which have no exactly corresponding representatives in the now existing plants. Colossal and luxuriant forms of vegetation, however, indicate a climate highly favourable to vegetable development. A careful examination of the petrifactions taken from these strata shows also so accurate an agreement with the fossil plants of the same period found in many parts of the Continent of Central Europe, that we are obliged to conclude that at that time no appreciable difference of climate existed on the face of the earth, but that a uniform climate extremely favourable for vegetation—but not on that account necessarily tropical—prevailed from the Equator to the Poles.

MECHANICS.

Browning's Patent Self-acting Latch.—One of the best—because it is at once the most efficient and withal the simplest kind of bolt that we have for some time seen—is the ingenious invention of Mr. Browning, which is figured below, and which has been recently patented. But besides its sim-



plicity, it has a special advantage, viz. that any attempt to open it from the outside renders it more securely fastened than it was before, while it registers the attempt made to open it by an alteration in its position. The bolt must be seen to be fully appreciated, and we doubt not it will soon become a most popular contrivance. It is certainly the most ingenious and yet the simplest invention in this direction that we have ever seen.

A Machine for Darning Stockings.—We have had sewing and knitting machines for some time, but the latest addition to our stock is that of a darning machine, which is described by the "Scientific American" as follows :—"Two small plates, one stationary and the other movable, are placed one above the other. The plates are corrugated, and between them the 'holy' portion of the stocking is laid. Twelve long-eyed pointed needles are arranged side by side in a frame, which last is carried forward so that the needles penetrate opposite edges of the hole, passing in the corrugations between the plates. Hinged just in front of the plates is an upright bar, and on this is a cross-piece carrying twelve knobs. The yarn is secured to an end knob, and then, with a bit of flat wire, pushed through the needle-eyes. Then the loop between each two needles is caught by the hand and hooked over the opposite knob, so that each needle carries really two threads. Now the needles are carried back to their first position, and, in so doing, they draw the threads, which slip off the knobs through the edges of the fabric. A little push forward again brings the sharp rear edges of the needle-eye against the threads, cutting all at once. This is repeated until the darn is finished, and beautifully finished it is. The cost of the machine is but ten dollars."

MEDICAL SCIENCE.

A New Cure for Sea-sickness.—This, which is the salt known as nitrite of amyl, has been recommended by Dr. C. Clapham. He says, in the "New York Medical Journal" (October 1875):—"As to the proximate cause of the malady, I entirely agree with Dr. Chapman that it consists of an undue congestion of the vessels of the spinal cord. On this point I had an excellent opportunity of drawing some conclusions from a post-mortem which I was fortunate enough to make while acting as superintendent of the Government Civil Hospital at Hong Kong last summer. The case was that of a Chinaman who had been killed while in the very act of vomiting during an attack of sea-sickness, by the fall of a heavy piece of iron from aloft. I found, on making the necropsy (four hours after death), that, leaving out of consideration the heart, which had been pierced by the falling iron, all the organs were healthy with the exception of the spinal cord, the vessels of which were literally gorged with blood throughout its entire length. I was struck with the similarity of this appearance to that presented by the spinal cord of an epileptic patient who died in the "status," and upon whom I made a post-mortem while at the West Riding Asylum, Wakefield. Coupling the post-mortem likeness to the resemblance which obtains in life between these two affections (pallor of surface, cold sweat, &c.), it occurred to me that the remedy which, in the hands of Dr. J. Crichton Brown, has proved so valuable in the epileptic "status," might be advantageously employed in the treatment of sea-sickness. To test the truth of this surmise, I made several trips across the Pacific, and tried the remedy altogether in 124 cases. Of these, 121 cases proved eminently satisfactory, there being no return of the vomiting after the administration of the nitrite; the remaining three cases being only unsatisfactory in so far as they required a further dose or two of the remedy." We fear, however, that a short experience of Dr. Clapham's remedy will show of how little use it is alone. Certes one of the best safeguards is lying perfectly recumbent (before the vessel starts) and keeping the body warm and taking a substantial meal about an hour before you set sail. The writer has tried these plans in a recent stormy passage across the English Channel, and he perfectly avoided sea-sickness. Yet on this occasion thirty or more people were violently sick, and, what is more extraordinary, he never before escaped sea-sickness in rough weather. One thing we had almost forgotten to mention, *i.e.* keep the eyes almost constantly closed, and be as nearly amid-ships as possible.

Giving Medicines to the Mother for the Suckling Infant.—Dr. Lewald has, says the "Lyon Medicale," investigated the elimination, by the milk of the mother, of iron, bismuth, iodine and its compounds, arsenic, lead, zinc, antimony, mercury, alcohol, and several narcotics. His numerous experiments were made in the goat. A certain dose of the medicine was administered to the animal, after which the milk was examined. The principal conclusions which the author has arrived at are:—1. A larger quantity of iron can be administered to the infant through the mother's milk than by any other means. 2. Bismuth likewise is eliminated by the milk, but in very small quantity. 3. Iodine does not appear in the milk until ninety-six hours after taking it; the iodide of potassium given in doses of forty grains *per diem* appears four hours after ingestion, and continues to be eliminated for eleven days. 4. Arsenic appears in the milk at the end of seventeen hours, and its elimination had not ceased after sixty hours. 5. Though one of the most insoluble preparations, the oxide of zinc is nevertheless eliminated by the milk, and it is probable that this is also the case with the other preparations of zinc; fifteen grains of oxide of zinc were found in the milk at the end of from four to eight hours, and it disappears sooner than iron, because no trace of it could be discovered after fifteen or sixteen hours. 6. The elimination of antimony is an undeniable fact, and it is well to bear this in mind during the period of nursing; the same holds true in regard to mercurial preparations. 7. That alcohol and the narcotics are eliminated by the milk has not been demonstrated. Sulphate of quinine is eliminated very easily; a child suffering from intermittent fever was cured by administering quinine to the nurse.

METALLURGY, MINERALOGY, AND MINING.

A Mineralogical Society is likely to be formed in London, and it is undoubtedly required. Mr. J. H. Collins, who is engaged in furthering the plans of this project, has written the following letter to the "Geological Magazine" (Nov. 1875). Dating from 57 Lemon Street, Truro, he observes: "An effort is being made for the establishment of a Mineralogical Society of Great Britain and Ireland." The objects of the Society are—

To simplify mineralogical nomenclature.

To determine and define doubtful mineral species.

To study the *paragenesis* of minerals.

To record instances and modes of pseudomorphism, with their accompanying phenomena.

To measure, determine, and illustrate forms of crystallization, especially the irregularities and peculiarities of particular planes, or of crystals from particular localities.

To discuss systems of classification, and to establish a natural system.

To collect, record, and digest facts and statistics relating to economic mineralogy.

To promote the exchange of specimens; and, generally,

To advance the science of mineralogy.

The rules and regulations to be ultimately adopted will be decided upon by the votes of probably the first 100 members.

Graphite from Siberia.—M. S. Kern, of St. Petersburg, writes to the "Chemical News" (Nov. 12, 1875) as follows:—

Having lately analysed two samples of Siberian graphite from the Stepanovsky Mine, I think the results of analyses may be of some use. The Zeilon graphite containing 80 per cent. carbon, and coming from England to Russia, is far richer and cleaner, and mixed with Russian graphite is used for the manufacture of black-lead crucibles.

Analysis of Stepanovsky Graphite.

	Samples No. I. Per Cent.	No. II. Per Cent.
Carbon	36·06	33·20
Silica	37·72	43·20
Perric oxide	4·02	3·05
Alumina	17·80	15·42
Lime, Magnesia	1·20	1·06
Volatile matter	3·20	4·03
Sulphur	traces	0·04
	100·00	100·00

METEOROLOGY.

Meteorology in England.—The report of the Meteorological Committee for 1874 has been quite recently published, and commented on by the "Academy" which says:—

The Report of the Meteorological Committee for the year 1874 has just appeared, and it shows that their office has carried out steadily work of the same nature as in former years. As regards marine meteorology, the number of observers remains small compared with the strength of our merchant navy, but the quality of the observations which are received appears to be good. The investigation of the nine 10-degree squares lying close to the equator in the Atlantic, is nearly complete, and monthly charts for this region will shortly appear. The next district to be attacked by the office is the south point of Africa.

The results of storm-warnings are much the same as in 1873, nearly 80 per cent. of the warnings having been justified by subsequent weather, and more than half of that proportion having been followed by serious storms.

In the land meteorology of the United Kingdom some important changes are noticed in the announcement of the satisfactory conclusion of arrangements for co-operation between the office and the Meteorological Society (of London), in virtue of which the Society will supply returns from certain selected stations for publication *in extenso* by the office in conjunction with returns from its own volunteer observers, and in accordance with the international plan proposed by the Permanent Committee of the Vienna Congress, which has already been noticed in these pages.

When the vote for learned societies was taken in the House of Commons, Mr. Maclagan moved its reduction by the sum of 1,000*l.*, this amount to be transferred from the Meteorological Office to the Scottish Meteorological Society, and was defeated; but Mr. Smith, on the part of the Treasury, stated that it was the intention of that department to institute an inquiry into the meteorological organisations of the country in the course of the autumn.

All meteorologists will hail this announcement as most satisfactory, as no such inquiry has ever yet been held except that before the Commission on the Advancement of Science now just closed, for the committee appointed in 1865 only reported on the state of the Meteorological Department of the Board of Trade.

The Report of the Commission just mentioned has also appeared, and has been noticed in our columns; but, as it goes at some length into the subject of Meteorology and the condition of the Meteorological Office, it may be allowable to refer again to its contents. The commissioners for the most part content themselves with reproducing the opinions of some of the gentlemen who have given evidence before them. In their remarks on the evidence, however, they say (p. 25):—

"With respect to meteorology, we are of opinion that the operations of the Meteorological Office have been attended with great advantage to science and to the country. The subject of Meteorology is a very vast one, and any scheme for its proper cultivation or extension must comprise—(1) arrangements for observing and registering meteorological facts; (2) arrangements for the reduction, discussion, and publication of the observations; (3) researches undertaken for the purpose of discovering the physical causes of the phenomena observed. The resources placed at the disposal of the committee are inadequate to cover the whole of this wide field; and, having due regard to all the circumstances of the case, we believe that in selecting parts of it, as the objects of their special attention, they have been guided by a sound discretion.

"We are also disposed to consider that although, as we have already said, the Meteorological Committee occupies an anomalous position, no other form of organisation could advantageously have been adopted under the actual conditions. We think, however, that if, as we shall hereinafter recommend, a Ministry of Science should be established, the head of the Meteorological Office should be made responsible to the minister."

The Commissioners further comment upon the views held by Professor Balfour Stewart and others, to the effect that climatological inquiries should be left to the efforts of local societies aided by Government; and while acknowledging the usefulness of the results already yielded by such systems in the United Kingdom—*e.g.* that of Mr. Glaisher and that of the Scottish Meteorological Society—they express their opinion that any grants in aid should be made on a systematic principle, which could best be effected by making them subject to the control of a minister.

MICROSCOPY.

Microscopical Papers for the Quarter.—The following articles have appeared in the "Monthly Microscopical Journal" for October, November, and December:—

On Cephalosiphon and a New Infusorion. By Dr. C. T. Hudson, LL.D.—Recent Progress of our Knowledge of the Ciliate Infusoria. By G. J. Allman, M.D., F.R.S.—Extracts from Mr. H. E. Fripp's Translation of Professor Abbe's Paper on the Microscope.—On a New Melicerta. By C. T. Hudson, LL.D., F.R.M.S.—On the Identical Characters of

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PHYSICS.

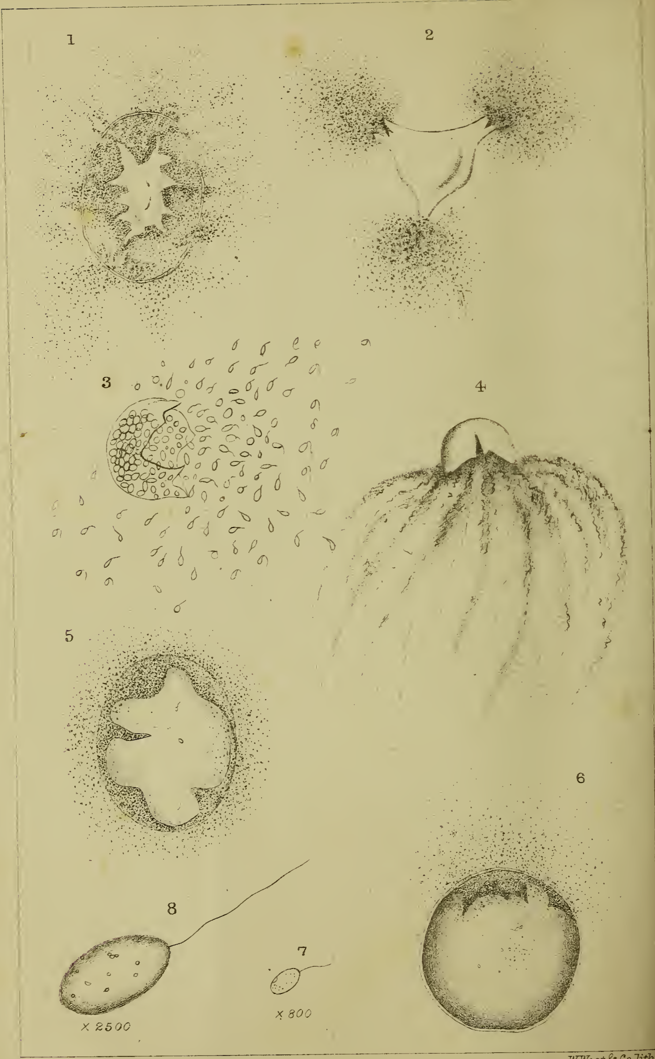
Death of Sir Charles Wheatstone.—Physical Science has sustained the deepest possible loss by the death of Sir Charles Wheatstone, F.R.S., which took place at Paris, at the Hôtel du Louvre, on October 20, 1875. Identified as he was with the practical development of telegraphy, the name of Wheatstone was familiar to all, and the fact of the later years of his life having been devoted to subjects connected with electricity has led even the scientific world to lose sight of his valuable work in other branches of physics. As early as 1823, while engaged in the manufacture of musical instruments, Charles Wheatstone published a paper on "New Experiments on Sound," and on looking through the list of his published papers one recalls the value of his experiments in connection with sound and light. Still it is as the electrician that Wheatstone rose to the first rank in science, and, as we have already said, it is the conspicuous part he played in the invention and application of the electric telegraph that raised him so high in popular esteem. In 1834 Professor Wheatstone was appointed Professor of Experimental Philosophy at King's College. In 1836 he was elected a Fellow of the Royal Society, when he read a paper entitled "Contributions to the Physiology of Vision," and this led to the invention of the stereoscope, which he first exhibited at the meeting of the British Association for the Advancement of Science in 1838. In 1868 Wheatstone was knighted in recognition of his scientific services, but thirteen years before the Emperor of the French had appointed him a Chevalier of the Legion of Honour on account of his application of the electric telegraph. He was also a Member of the French Academy. A list of his principal papers, contributed to scientific societies and journals, has been published in the "Chemical News," where it occupies nearly a whole column.

Experiments on the Ultra-Violet of the Spectrum.—In a late number of the "Comptes Rendus" (October 18), M. Croullerois states that in order to effect the measurement of the rotatory power of quartz in the ultra-violet of the spectrum, he had recourse to the procedure of Mr. Stokes, and made use of the analytical method of MM. Fizeau and Foucault; but, notwithstanding the most favourable atmospheric conditions, it was impossible for him to carry the measurement of the rotations beyond the ray O. It is true that in his experiments the normal ultra-violet spectrum, and the interference band, were seen by reflection on paper saturated with the fluorescent solution, the eye interfering only in a secondary mannner in the observation of the phenomenon. When M. Soret made known to the author his fluorescent eye-piece, with which the spectrum may be regarded under conditions of direct transmission, apparently more favourable, he pointed out to him this application of his ingenious instrument. The author has noticed, in the "Comptes Rendus" of October 11, a paragraph showing that this application has been successfully tried by MM. Soret and Sarazin; but these physicists have not been able to extend their observations beyond the ray N, which shows that the absorption by transmission is still more injurious than the loss of light by epipolic dispersion.

ZOOLOGY AND COMPARATIVE ANATOMY.

The Anatomy of the Giraffe.—Although this subject has been examined before by Professor Owen, F.R.S., Dr. H. C. Chapman has published some observations on it before the Academy of Natural Sciences of Philadelphia. They were made upon an animal which died a short time ago in the Philadelphia Zoological Gardens. He had pleasure in saying that he found the internal organs as described by Professor Owen, save in reference to the manner in which the great blood-vessels spring from the aorta. In the example dissected by Dr. Chapman there was an innominate artery which gave off the left subclavian, the right subclavian, the right vertebral and the common trunk of the carotids, the left vertebral springing alone from the aorta; whereas in Professor Owen's example, according to the description, the left subclavian, as well as the left vertebral, came off separately from the aorta, while the right vertebral came from right subclavian. It is possible that in the former the disposition of the blood-vessels was an anomalous one. He would also mention that there was an entire absence of a gall-bladder, which was noticed twice out of three times in the cases studied by Professor Owen. For the reason above given he did not refer to the brain, alimentary canal, &c.; to those who may be interested, he would simply state that these organs may be seen in the museum of the University of Pennsylvania.

The Meteorological Aspects of the Flight of Grasshoppers.—In some parts of the Southern States of America great flights of grasshoppers take place at certain seasons; and a curious fact has been recently observed in connection with their flight, which would seem to give them the power of forecasting the state of the weather. Mr. J. Wilson states that lately, on a cloudy afternoon, the insects were on the wing, high in the air, in countless multitudes. A party of several persons was riding in a carriage, and the question of probable rain was discussed. Suddenly the grasshoppers, with great unanimity, descended to the ground, the scene reminding one of a furious snowstorm. In two or three minutes no grasshopper could be seen in the air, and in a short time it commenced to rain. Soon after the rain ceased to fall the insects took flight again, but in the course of half an hour, without any particular indication of rain, they suddenly plunged to the earth again. Soon after this it rained again. This process was repeated three times on that afternoon, and each descent was followed by a fall of rain.



W. H. Dallinger delt.

W. West & Co. lith.

Is there spontaneous generation?

PROFESSOR TYNDALL'S EXPERIMENTS ON SPONTANEOUS GENERATION, AND DR. BASTIAN'S POSITION.

By the Rev. W. H. Dallinger, V.P.R.M.S.

[PLATE CXXXIII.]

IN the present position of Biological Science in relation to this important and interesting question, any positive results which have a definite bearing on the difficulties of the subject, and point hopefully to new methods of research, must be warmly welcomed. Professor Tyndall's beautiful series of experiments "On the Optical Deportment of the Atmosphere in reference to the Phenomena of Putrefaction and Infection" are precisely of this class, and will give new impulse and direction to all unbiassed labour. It is to be regretted when, in a matter so purely one of rigid science as this is, impassioned controversy is suffered to have any place. It fails utterly of its intended purpose, and simply hinders and delays the final issue. There are few but will have admired the animation, courage, and resolution manifested by Dr. Bastian in the discussion of this question during the last five years; but those who have been most capable of understanding the method, nature, and object of his experiments, and the general drift of his reasoning, are those who most earnestly disavow the perhaps unconscious, but nevertheless too palpable, advocacy of a *thesis* which his writings so freely display.

Dr. Bastian's position in relation to the origin of minute organic forms has, at the outset, the immense disadvantage of being adverse to the whole analogical teaching of nature, down to the uttermost depths of minuteness, *where our knowledge is accurate and sound.* Wherever science has put down the landmarks of possession, and is not dealing with the disputable territory of hypothesis, it is absolutely known that at some period in the cycle of development the lowliest organisms are dependent for their propagation upon what we can only look upon as genetic products.

Manifestly, then, it must be weighty—nay, unequivocal and even irresistible—evidence that will induce the philosophical Biologist to conclude that nature's otherwise universal method is changed, in the outmost fringe of organised being. Mere reasoning could never accomplish this. It must be hard, defiant fact, which none can gainsay. But verily no such facts—nor even their most distant forecasts—are before us. The profound difficulties which bristle round the enquiry on every hand are prominent signals for caution; while the uncertainty and incompetency of the methods hitherto employed, and their conflict of results, is alive with meaning. Indeed, we are dealing with organisms so minute as to elude all but our best optical appliances; and the accurate and correct interpretation of the details they enable us to discover requires the practice and experience of years. Of the developmental history of these organisms themselves, we know from actual observation almost nothing with certainty; and the little we do know from such careful and patient observers as Cohn, Billroth, Ray, Lankester and others, is so complex and conflicting as to demonstrate the necessity of years of patient experiment and skilled research; and to plainly tell us of our ignorance of this minute and wonderful group of organic forms. And yet, forsooth, we are asked, upon the conflicting testimony of a multiplicity of boiled infusions, yielding often even in the same hands uncertain results, and in different hands conflicting ones, to believe that organic nature—whose method of reproduction is the same to the very limits of *certain* knowledge—changes its method in this uncertain and cloudy region.

Of course to "spontaneous generation" as a mode of vital reproduction there can be no *à priori* objection. Let us have it by all means, if it be a fact in nature; but not on any other terms. Is it reasonable to suppose that such men as Darwin, and Huxley, and Tyndall, and Burdon Sanderson, and Cohn, and Billroth, and Lankester, would shrink from "spontaneous generation" because of the "consequences" to which, strangely enough, it is by some supposed to lead? The very thought admits of nothing but ridicule. And yet Dr. Bastian is displeased with Darwin * because he has not definitely determined whether all living things originated in one primordial germ, or originated spontaneously in multitudinous centres scattered over the earth's surface. Both Huxley and Tyndall are in effect charged with grave inconsistency,† because, while they admit the origin of all vital forms by evolution, they

* "Evolution and the Origin of Life," pp. 13–17. † Ibid. pp. 15–16.

yet declare that they have never *seen* an instance of "spontaneous generation" of organised forms. It is asked "Why should men of such acknowledged eminence in matters of philosophy and science as Mr. Herbert Spencer and Professor Huxley promulgate a notion which seems to involve an arbitrary infringement of the uniformity of nature?" I dare not answer for them; but for myself I answer, Because the facts as presented to them on the subject—as well known to them as to Dr. Bastian, and we may venture to say as well considered—do not appear to involve the "arbitrary infringement" of nature's uniformity of which Dr. Bastian speaks. If these admittedly competent and proverbially fearless men could be led by facts to see that their teaching promulgated an "arbitrary infringement" of nature's method, is it rational to suppose that they would persist in it another hour? The very position, therefore, of the leading biologists of the day in relation to the hypothesis of "spontaneous generation" is an authoritative declaration of the invalidity of the data on which it rests.

To Dr. Bastian, nevertheless, the "facts," such as they are, have carried a different conviction. But on analysis, that conviction is evidently not wholly formed upon the bare "facts." It is influenced and stimulated by a "philosophy" which, in short, is this:—Continuity in nature is the grand outcome of all modern research; but if you are to have this in a sense wide enough to include the organic world, you must have "spontaneous generation." Give up this, and continuous evolution is impossible; *therefore* abiogenesis *must be* a great truth.

Of course continuity in nature is a profound truth. Every careful and comprehensive student of modern biology will admit that. By Dr. Bastian's own showing, Huxley, Darwin, and Spencer are its most competent expositors. But they prefer not to be hasty. They decline to determine the exact manner or line of that continuity *until they have facts of a competent kind to guide them.* There may be lines of continuity infinitely more subtle than any the subtlest minds have even conceived. At least they decline to accept one, laid down, as it appears to them, not by nature, but by Dr. Bastian; and no believer in the evolution of living things, surely, is recreant of his creed who declines a similar surrender.

The largest difficulty surrounding the question of the mode of origin of septic organisms is that of discovering their life-cycle. By dealing with them in aggregations we run told and untold risks. The conflict of results by this means, in the most accomplished hands, employing the most refined methods during the past eighteen years, is a sufficient witness. Repetitions of experiments, and conflicting results, and explanations of the reason why; and so the cycle rolls. Of course important lessons in biology are learned, but not *the* lesson. And yet by the teachings of this complex and doubtful method *alone* Dr. Bastian is content to accept "abiogenesis" as a great fact in nature.

To those who are best acquainted with the experimental history of the subject for the last twenty—but certainly for the last six—years this is the more remarkable. For the weight of evidence is certainly not only *not* in favour of "abiogenesis," but is in the strongest sense adverse to it. The most refined, delicate, and continuous researches all point to the existence of what are at present ultra-microscopic germs. This, indeed, is directly affirmed by the authors. A single and recent instance will suffice. After a remarkable series of experiments detailed before the Royal Society Dr. W. Roberts says: "The issue of the foregoing inquiry has been to confirm in the fullest manner the main propositions of the panspermic theory, and to establish the conclusion that *bacteria* and *torulæ*, when they do not proceed from visible parents like themselves, originate from invisible germs floating in the surrounding aërial and aqueous media."*

But further, this has been remarkably sustained by analogical evidence. There are putrefactive organisms that closely approximate to the bacteria in form, structure, and size. These are the "*monads*," or, as Professor Huxley doubtless more fitly names them, the *heteromita*.† They live side by side with the bacteria in the same putrescent mass, and certainly in the later stages of the disintegration of dead organic matter are the most active and powerful agents. From their greater size they present a more promising field for microscopical research than the bacteria themselves; and the life-history of some of these could be fully mastered. I long since felt that valuable aid might thus be rendered to the discovery of the nature of the bacteria. Armed with the best and most powerful appliances which the modern optician could supply, Dr. J. Drysdale and myself ventured on the work. The results are fully detailed elsewhere.‡ It need only be remarked here that the only hope of success was in *continuous* observation of the same form, in the same drop of fluid, under the highest powers. The secret, therefore, was to find a means of keeping the same drop under examination without evaporation. This we did. § The result was that patient work enabled us to completely unravel the life-history of six of these organisms. These life-cycles cannot be here recounted. Suffice it now to say that each of them multiplied enormously

* "Phil. Trans." 1874, p. 475.

† "Macmillan's Magazine," Feb. 1876, p. 379.

‡ "Monthly Micros. Journ." vols. x. xi. xii. and xiii.

§ Ibid, vol. xi. pp. 67–69.

by self-division (fission), but that the life-cycle in *each case* began and ended in *a distinct genetic product*—call them what we choose, spores, germs, or ova.

In Pl. CXXXIII. I have drawn from nature, in the six respective cases, the condition presented by each organism at the time of emitting its spore. Fig. 1 is the genetic product of an oval monad, with a pair of flagella; it rapidly increased by fission; then in a remarkable manner a pair blended, became *one* in the form of a sac, the sac burst and poured out, as the drawing portrays, innumerable spore, which were watched continuously until they were seen to develope into the parent condition. Fig. 2 gives a similar product of another form, different anatomically and in all the details of metamorphosis, but yet passing through the states of fission, blending into a sac, and (as seen) the emission of spore; which were again watched into the parent condition. Fig. 3 shows the direct genetic product of a third, but this sac did not contain spore, but *living young*, which swam forth at once upon the bursting of the sac; and by taking in pabulum at all points of the sarcode, rapidly grew to the parent size. In fig. 4 we have new features. The organism is oval, with one flagellum. It multiplies with enormous rapidity by *multiple* fission,* and then by distinct genetic union a sac is formed and spore emitted; but they are packed in a glairy fluid, and were so minute that at first our best powers failed to reveal them. But they were afterwards seen, and their full development traced. In figs. 5 and 6 we have the same products of the two last monads. In morphological detail they greatly differed from all the preceding ones, and from each other. But the spore-sacs were produced by the same means, and the exquisitely minute spore poured forth were traced through all their stages to the adult condition.

We have here, then, important indications of fact concerning the nearest allies of the bacteria: they develope from germs.

We have besides, the weight of the best experimental evidence pointing clearly to the existence of germs in the bacteria themselves. But the microscope has failed to *demonstrate* the latter. Its finest powers and finest methods failed to reach them.

Happily at this juncture Professor Tyndall has stepped in, and, with his accustomed brilliance and precision, has opened up the path we need. *He has presented us with a physical demonstration of the existence of immeasurably minute molecules of matter—utterly beyond the reach of the most powerful combination of lenses yet constructed—which are the indispensable precursors of bacteria in sterilised in-*

* "Monthly Micros. Journ." vol. xi. pp. 69–70.

*fusions.** In short, he has opened up a new and exact method, which must lead to a scientific determination of the existence and nature of the bacteria-germs. His beautiful experiments on the decomposition of vapours, and the formation of actinic clouds by light, led him to experiment on the floating matter of the air, and with what results is widely known. Confined and undisturbed air, however heavily charged with motes, becomes at length, by their deposition, absolutely clear, so that the path of the electric beam is invisible across it. From this, and associated indications, he acutely inferred "that the power of developing life by the air, and its power of scattering light, would be found to go hand in hand;" so that a beam of light sent across the air into which infusions might be placed and examined by the eye, rendered sensitive by darkness, might be utilised with the best results in determining the existence of bacteria-germs. To bring the idea to a practical result a number of chambers were constructed with glass fronts. At two opposite sides facing each other a couple of panes of glass were placed to serve as windows, through which the electric beam might pass. A small door was placed behind, and an ingenious device was arranged to enable a germ-tight pipette to have free lateral, as well as vertical, motion. Connection with the outer air was preserved by means of two narrow tubes inserted air-tight into the top of the chamber. The tubes were bent several times up and down, "so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air."

Into the bottom of the boxes were fitted air-tight large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

"On September 10 the first case of this kind was closed. The passage of a concentrated beam across it showed the air within it to be laden with floating matter. On the 13th it was again examined. Before the beam entered, and after it quitted the case, its track was vivid in the air, but within the case it vanished. Three days quite sufficed to cause all the floating matter to be deposited on the sides and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air."

In this way the air in its normal condition was freely supplied to the infusions, but of mechanically suspended matter it could

* "Nature," Jan. 27, 1876, p. 252; and Feb. 3, p. 268.

be demonstrated that there was none. And it was proved, with a clearness that admits of no quibble, that infusions of every kind, animal or vegetable, were absolutely free from putrefactive organisms. "In no single instance . . . did the air which had been proved moteless by the searching beam show itself to possess the least power of producing bacterial life or the associated phenomena of putrefaction." But portions of the same infusions exposed to the common air of the Royal Institution Laboratory at a continuous temperature of from 60° to 70° Fahr. fell invariably into putrefaction; and when the tubes containing them amounted to 600 in number not one of them escaped infection—they were all "infallibly smitten." Here is irresistible evidence that there is a direct relation between a mote-laden atmosphere and bacterial development. The whole series of Dr. Tyndall's exquisite experiments is simply an irrefragable affirmation of this truth. The presence of the physically demonstrated motes is as essential to the production, in a sterilised infusion, of septic organisms, as light is to actinic action. They cannot be made to appear without the precursive motes; they cannot be prevented from appearing if the motes be there. That these are the germs of bacteria by themselves, or associated with minute specks of matter, approximates to certainty in the proportion of hundreds of millions to one.

A beautiful illustration of the minuteness and multitude of the particles is given. Let clean gum mastic be dissolved in alcohol, and drop it into water; the mastic is precipitated and milkiness is produced. Gradually dilute the alcoholic solution, and a point is reached where the milkiness disappears, and by reflected light the liquid is of a bright cerulean hue. "It is in point of fact the colour of the sky, and is due to a similar cause—namely, the scattering of light by particles small in comparison to the size of the waves of light."

Examine this liquid with the highest microscopical power, and it appears as optically clear as distilled water. The mastic particles are almost infinite in number, and must crowd the entire field of the microscope; but they are as absolutely ultra-microscopic as though they had no existence. I have tested this with an exquisite $\frac{1}{50}$ of Powell and Lealand's, employed with a new and delicate mode of illumination for high powers,* and worked up to 15,000 diameters; but not the ghostliest semblance of such particles was seen. But at right angles to a luminous beam passing among these particles in the fluid "they discharge perfectly polarised light." "The optical deportment of the floating matter of the air proves it to be

* *Vide* "Monthly Micros. Journ." April 1876.

composed, in part, of particles of this excessively minute character," and it is among the finest of these ultra-microscopical particles that Professor Tyndall finds the sources of bacterial life. It is almost impossible to conceive a nearer approach to certainty concerning the nature of these minute particles than this. Their minuteness, their capability of being physically demonstrated, the absolute necessity of their presence to the origination of bacteria in sterilised infusions of any and every kind, taken in connection with what we *know* concerning the germs of the *heteromita* whose life-histories have been studied, render it simply inevitable that we have at length reached, what we are justified in believing to be, a genetic product of the bacteria through which their continuation as organisms is preserved. When first I saw the simplicity and beauty of this method, it struck me that its applicability as a test in reference to germs—*known to be such*—would have considerable collateral weight; and a method of employing it was suggested by a fact in past experience.* I had in my possession a maceration of cod's head, which I had kept in use for eleven months. It had become a pulpy mass, and in the middle of January last it was comparatively free from bacteria, but swarmed with two monads—the fourth and sixth of the series described by my colleague and myself. To ascertain their exact condition, I watched them on the "continuous stage" for three consecutive days, and found that both forms were to be seen plentifully emitting spore. The maceration had become very short of moisture, which served my purpose. I subjected it to a dryer air with a higher temperature, and it was not very long in becoming a moist pulpy mass, with sufficient cohesiveness to be removed from the vessel; and in this condition it was placed in a heating chamber, which was slowly raised to a temperature of 150° Fahr., and kept at this for an hour. This was 10° Fahr. higher than Dr. Drysdale and myself had proved necessary to destroy absolutely every adult form. The baked mass now appeared cracked, porous, and flaky. In parts it was extremely friable, and with little pressure crumbled into almost impalpable powder; while by friction a very large proportion was reduced to the finest dust. To avoid all possibility of error this powder was again exposed in the heating chamber, spread over a plate of glass, to a temperature of 140° Fahr. for ten minutes—thus rendering the plea of mere desiccation impossible.

A chamber or box was now prepared precisely like Professor Tyndall's, except that there were no tubes to communicate with the outer air.

* *Vide* "Monthly Micros. Journ." vol. xii. pp. 262–3.

In the "Researches" on the life-history of monads we had proved that they could live, thrive, and multiply almost as well in Cohn's "nutritive fluid" as in the normal animal infusion. This fluid is composed of phosphate of potash, sulphate of magnesia, triple basic phosphate of lime, tartarate of ammonia, and distilled water. If these ingredients are all mingled the fluid becomes speedily charged with bacteria, unless hermetically sealed, and sometimes even then. We therefore keep the ammonia in a separate solution, mixing them when required.

A portion of the fine dust of the maceration was now taken and thoroughly scattered through the air of the prepared chamber. The condensed beam from an oxyhydrogen lime-light* was then sent through it. Its line of passage was far more brilliantly marked inside the chamber than in the outer air. It was deemed inexpedient to insert the fluids while such brilliant points were visible in the air, and four hours were suffered to elapse. The lime-light beam was still visible with perfect distinctness, but its path within the chamber was much less brilliant and more homogeneous than it was without. The fluids were then carefully mixed, and five small glass basins of the mixture were inserted. The whole was undisturbed for five days. At the expiration of that time the beam of the lime-light sent through the chamber was absolutely invisible, although perfectly clear in the open air on both sides of it.

The fluids were now withdrawn. Ten "dips" were taken out of each basin for microscopical examination. *In every "dip"*—that is, fifty in all—*one or other of the monads appeared, and were in a state of active fission*; and in twenty-seven of the "dips" both monads were found. Bacteria swarmed the field, which of course I fully expected.

I now took five *other* glass vessels, and inserted them with great care into the now moteless air of the chamber, and poured in, as before, fresh Cohn's fluid. They were exposed for another five days. On careful microscopical examination of seventy-five "dips" *not a single monad of either form appeared*; bacteria were feebly present, but of course no steps were taken to guard against these, and, as before, they were anticipated.

The air of the chamber was again impregnated with dust, as before suffered for a time to settle, and *these same vessels of fluid*, which had yielded negative results, were again placed in the chamber. At the expiration of five days they were again examined, *and one or other of the monads was found in every successive "dip."*

Now let it be observed that there can be no possible error as

* This was of course very much less capable of "searching" than the electric beam; but it served for the rougher end I had in view.

to the forms. They were the identical species of the maceration, with which I am as familiar as with a barn-door fowl. What, then, is the logic of these facts? Dr. Tyndall proves that bacteria only develope in sterilized infusions when the air around them is laden with motes of incalculable multitude and exquisite minuteness. Given the presence of these, and the development of bacteria is inevitable. The inference is that the motes are *germs*. The above experiments show, that in closely allied septic organisms, the germs of which have been demonstrated and their developments watched, if the dry *dêbris* of a maceration in which these forms are found be scattered in the air around a prepared fluid, and demonstrated by similar optical means, that the said organisms develope; but if the minute dust from the *dêbris* be optically proved to be *absent*, none of the monad forms appear. Here we do not hypothecate a germ, but we *know* that it exists; and its deportment in similar conditions is identical with that of the assumed bacterial germ. Do we need more irresistible evidence that the bacteria develope, not *de novo*, but from genetic products?

Evidently Dr. Bastian thinks we do. He tells us in effect that if Dr. Tyndall has not succeeded, others have, in seeing bacteria reappear in infusions that have been exposed to a boiling heat for five minutes. This is true; but not to the extent nor with the meaning Dr. Bastian claims. He furnishes a list in "Nature"* for example, of those who are supposed to have secured the results he insists on. But this list is, perhaps hastily, but in effect, most unjustly framed. It is not surprising to see strong protests from the investigators concerned.† The citing of Roberts, for example, or Lankester and Pode, or Pasteur or Schwann, is simply a meaningless exercitation to all but the ignorant. Stripped of all disguise, the number of cases of the appearance of bacteria in sealed infusion after five or ten minutes boiling is few and doubtful indeed. But still there *are* cases, and in one instance at least admirably attested; but they are confessedly exceptional in a high degree. Dr. Bastian, however, prefers to interpret nature from the exceptional flasks, and infer "spontaneous generation" rather than be guided by the cumulative and overwhelming evidence of the existence of bacterial germs, as the medium of their normal reproduction. This must mean either that he believes that these organisms originate *de novo as well as* by germs, which is a direct *petitio principii*; or else that he is incapable of seeing the force of the facts which render the existence of germs inevitable. From the conflicting evidence of his own writing it would almost appear that he endeavoured to maintain

* Feb. 10, 1876. † E. G., "Nature," Feb. 24, 1876, p. 324.

both these views. He has recently said, "Professor Tyndall's results, admirable as they may be in themselves, *are altogether collateral, and do not bear upon the main point at issue.*" * Surely the "main point at issue" is the mode of origin of bacteria, and we cannot get much nearer the origin of an organic form than by tracing it to a genetic product—a spore! This *was* originally Dr. Bastian's question—did bacteria originate *de novo*, or from parents? It is not so now. He says, "*The* question is, not what air does or does not contain, since I have long ago shown . . . . that boiled fluids can be made to putrefy and swarm with bacteria in closed flasks, from which air and whatever it may contain has been expelled." † The same reasoning also obtains in his communication to the "Lancet" ‡ and to "Nature." § The result is clear. The doctrine of "spontaneous generation" rests upon *exceptions* for its truth. In rare instances, and in special infusions, bacteria have appeared after prolonged boiling. After a careful sifting of the evidence, the meagreness of the testimony is striking. All that can be fairly taken at all, when justly weighed, if taken altogether, is not equal to the evidence given by Dr. Burdon Sanderson. ‖ But it is well known that, while admitting and publishing the facts, he ignores absolutely Dr. Bastian's inference. And surely this is the truer philosophy. Let it be granted that by means not now explicable, the germs of bacteria, destructible in filtered infusions at a boiling temperature, are feebly, and at times, able to survive a slight continuation of the boiling point in infusions containing solid particles without apparent injury, is not that a ground for enquiring the reason why, rather than for inferring "spontaneous generation?" If we can prove that in 99 cases out of 100 *actual germs* are destroyed at 212° F., but that, in exceptional circumstances, the remaining one case yields bacteria *after* exposure to 212° F. for some minutes, is not that a reason for inferring, and looking for, some protective influence upon the germ, rather than launching into an hypothesis of an new mode of origin?

That the medium in which minute organic forms are subjected to heat exerts an influence on their subsequent deportment I can abundantly prove. I am equally convinced that the death-point of bacteria germs hovers very near the boiling point of water—a conviction amply sustained by fact. This being so, the survival, as germs, of some few, amidst incalculable myriads, by some accidental protection, is surely possible. So that, indeed, all true work now should be a study of the *germ* and its

* "Times," Jan. 29, 1876. † Ibid.

‡ Feb. 5, 1876. § Feb. 10, 1876.

‖ "Nature," Jan. 9, 1873, vol. vii. and vol. viii.

properties, and a discovery by patient research of the life-history of the organism.

The valueless nature of mere temperature experiments on such organisms, as tests of their ability to survive, without a knowledge of their life-history, Dr. Bastian, *without knowing it*, has made sufficiently plain. He gives a brilliant illustration—styled by himself "typical"—of the futility of his own method. Consider the facts.

In our "Researches" on the monads, my colleague and myself made it a special point to institute a series of investigations on the points of temperature which the adults, and the spore, of each form studied could resist. The results were as unexpected as they were remarkable. Only the results can here be stated. Taking the spore-sacs of the several forms in the order in which our illustration gives them, the data are as follow—viz. fig. 1 survived after exposure to 250° F.; figs. 2 and 4, 300° F.: fig. 3 (which produced living young), 180° F.; figs. 5 and 6, 250° F. That is to say, the spore, *after the heating to the above-named temperatures*, were followed step by step until they reached the parent condition. The adults of each form were absolutely destroyed at from 130° to 140° F. Thus, if all the examples be taken together, it will be seen that on the average the spore have a capacity to resist heat better than the adult in the proportion of 11 to 6. This is surely important.

Now, until Dr. Bastian's promised "new results"* have appeared, I believe I am justified in affirming that the strongest cases on which even he relies for "spontaneous generation" are recorded on pp. 175–180 of his "Evolution and the Origin of Life." They are thus introduced:—"After this I may, perhaps, be deemed fully justified in quoting two *very typical* experiments for the further consideration of those who stave off the belief in spontaneous generation—either by relying on insufficient reasons for doubting the influence of boiling water, or because of their following Pasteur, Cohn, and others in supposing that certain peculiar bacteria germs are not killed except by a brief exposure to a heat of 227° or 230° F. For even if we could grant them these limits, of what avail would the concession be . . . . in the face of the following experiments?" The details of the experiments follow. They are alike in method, and we will concern ourselves only with the second. A strong infusion of common cress, with a few of the leaves and stalks of the plants, were inclosed in a flask, which was hermetically sealed while the fluid within was boiling. It was then introduced into a digester and gradually heated, and afterwards kept at a temperature of 270–275° F. for twenty minutes, and was

* *Vide* "Times," Jan. 29, 1876.

retained at a temperature, if the time of heating and cooling be considered, over 230° F. for one hour. This flask was opened after nine weeks. The reaction was acid; the odour was not striking. On microscopical examination with a $\frac{1}{12}$ inch objective "*there appeared more than a dozen very active monads.*"

Now, fortunately, Dr. Bastian has not only carefully measured and described these organisms, but he has drawn them, and they are reproduced on the frontispiece of the book. He describes them as the 1-4000th of an inch in diameter; they were provided with a long, rapidly moving lash (flagellum), by which granules were freely moved about. But, besides this, "*there were many smaller, motionless, tailless spherules, of different sizes, whose body-substances presented a similar appearance to that of the monads*—and of which they were in all probability earlier developmental forms." *

Now, by careful comparison, I find that this monad is no other than the "uniflagellate monad," which is the fourth in the series whose life-histories were studied by Dr. Drysdale and myself.† Figs. 7 and 8, Pl. CXXXIII., will help to make this clear, where fig. 7 is an exact rendering of Dr. Bastian's monad magnified 800 diameters; and fig. 8 is a drawing of the "uniflagellate monad" described by my colleague and myself, magnified 2,500 diameters. We describe it thus:—"Its exterior form is extremely simple, being ovoid, with a single flagellum. Its long diameter never exceeds the 1-4000th part of an inch" in length.‡ Now, from a very prolonged and careful study of these organisms, I am convinced that Dr. Bastian's form and ours are absolutely identical. But to make the thing simply irresistible we have further and final evidence. One of the metamorphoses of this monad on its passage to multiple fission is that it loses its flagellum, and becomes precisely what Dr. Bastian saw all around—a motionless spherule." § These little bodies are less in diameter than the active monad, and of precisely the same structure. The identity is thus complete. The evidence is as full as may be; the monad Dr. Bastian saw was the one whose life-history was fully worked out. As usual, it multiplies by fission, but the fission is multiple. It then passes to a sac-like condition, resulting from the uniting together or fusion of two individuals. This sac becomes still and bursts, as seen in fig. 4, pouring out spore that taxed our highest powers and closest watching. The spore of only two of the monads studied survived after exposure to a temperature of 300° F. *This is one of them.*

Now, Dr. Bastian says, "A drop of the fluid containing several

* "Evolution," p. 178. † "Monthly Micros. Journ." vol. xi. p. 69, *et seq.* ‡ P. 69, ibid. § P. 69, ibid.

of these active monads was placed for about five minutes on a glass slip in a water oven, maintained at a temperature of 140° F. All the movements of the monads ceased from that time, and they never afterwards showed any signs of life."* *This is precisely our experience.* But now mark the reasoning. This monad was killed at 140° F., *but it was found in an infusion that had been heated up to* 275° F.; THEREFORE *it must have originated de novo.*

But it has been shown that the monad has germs, and that these have a power of resisting heat up to 300° F.—that is to say, 25° F. higher than that to which Dr. Bastian's infusion *was* exposed—and *therefore*, by the logic of facts, the monads found were not a result of "spontaneous generation," *but were the natural outcome of a genetic product contained in the infusion, and which the heat employed could not destroy.*

We need no stronger proof of the futility of reasoning concerning the thermal death-point of a minute organism where developmental history is wholly unknown. Yet so confident is our experimenter of his result that he says: "Nothing that has yet been alleged, by way of objection to the admission of spontaneous generation as an everyday fact, at all effects such experiments as these. The shortest way out of the difficulty would, therefore, be to doubt the facts." But I think I have shown a still shorter way "out of the difficulty," and that, without the discourtesy of doubting Dr. Bastian's experimental "facts."

The truth, then, is that Dr. Bastian had no real knowledge of the monad; but he argued as if he had. Hence assumed premises led to a false and fatal conclusion.

He is simply repeating this in his latest attitude in reference to the question of the mode of origin of bacteria. Compelled to yield all else, he throws up a rampart round his exceptional flasks, and declares "spontaneous generation" to be impregnable—an inviolable law of nature. Dr. Tyndall is plainly told that his knowledge is insufficient, that he has mistaken the meaning of the question, and that his mode of treating it is "laughable;" † and all this arises from the fact that Professor Tyndall dealt with the question of *the mode of origin* of bacteria generally; whereas, to have pleased Dr. Bastian, he ought to have explained some exceptional conditions to which he now points—the exceptions being more important than the rule!

What are the facts?

I. Dr. Tyndall has proved, in connection with a host of others, but in a more definite and precise manner, that in *filtered infusions* five minutes' boiling does kill every form of bacteria.

* "Evolution and the Origin of Life," p. 179.

† "Lancet," Feb. 5, 1876; and "Brit. Med. Journ." Feb. 5, 1876.

II. He has further shown that they are propagated by demonstrable germs *only*, in such infusions ; and

III. This fact removes the probability of their spontaneous generation to an almost infinite distance.

As to the development of bacteria in infusions charged with solid matter, precise experiment of a sufficiently comprehensive character has yet to be made on them, in relation to the demonstrated germs. Meantime, shall we accept "spontaneous generation" on such ground as its strongest advocate has now to offer, and ignore the vast chain of facts copiously attested and controlled, which are in perfect harmony with the known laws of the entire organic world ? This, and nothing less than this, is what Dr. Bastian inculcates and demands.

EXPLANATION OF PLATE CXXXIII.

Fig. 1. Spore-sac of the cercomonad—the first in the series of Messrs. Dallinger and Drysdale's "Researches"—emitting spore.

Figs. 2, 3, 4, 5, and 6. The same states of the five other monads of the series.

Fig. 7. A monad found by Dr. Bastian in an infusion, after heating up to 275° F., said to be spontaneously generated.

Fig. 8. The same monad as seen by Dallinger and Drysdale, and the spore of which (fig. 4) survives 300° F.

HEAT AND NOT LIGHT A MOTIVE POWER; OR, EXPERIMENTS WITH RADIOMETERS.

By H. A. CUNNINGTON.

THESE ingenious and beautiful little instruments were invented by Mr. Crookes. I believe he at first supposed that they showed "repulsion by radiation" of heat; since then he speaks of them as showing the "mechanical action of light," and has gone so far with his experiments as to *weigh* a ray of light, proving that the *force* of the light our earth receives from the sun is equal to fifty-seven tons per square mile. I have been experimenting for nearly three months with some of the radiometers made by Dr. Geissler, and obtained through Mr. Browning, who has kindly placed at my disposal some of the best he has had. As these little instruments are not generally known, I will try and give a description of them before beginning an account of my experiments. Four little arms of equal length are fastened at right angles to one another, and attached to a socket of hard glass, in which the point of a needle works as a pivot. On the free end of each arm is fastened a vane, black on one side and white on the other, each one of the same size, and so placed that they shall be like a continuation of the arms, and be at right angles to the plane of revolution. These arms, vanes, socket, and needle are enclosed in a glass globe, with a hollow stem, which serves as the means of keeping the globe upright by fixing it in a hole in a short wooden stand. The air is exhausted from the globe by means of a Sprengel air-pump until the nearest approach to a perfect vacuum has been obtained. All my experiments seem to point to the conclusion that "heat" is the motive power, and not "light"; that is, if they can be considered as separate agencies. A theory of the action of the heat has been suggested to me by my brother, Mr. Cecil Cunnington, which seems to be able to account for all the phenomena I have as yet observed. I will not give it until after the account of my experiments, as it will then perhaps be more intelligible.

I have two radiometers exactly alike in make, with the exception that one, which I shall call A, has diamond-shaped vanes, of which the reflecting sides are not so effective as the absorbing sides; and in the other, B, the vanes are round, with very effective reflecting surfaces, but indifferent absorbing surfaces.

When A and B are placed, say, six inches from any source of light, as a candle or in daylight, the black discs in A are repelled much quicker than those in B—that is to say, the rotation is much quicker in A than in B. This difference would be partly caused by the difference in black area in the two shapes of the vanes, but other reasons for it will be mentioned hereafter. I shall class my experiments under two sections. Under the first, I will describe those in which heat acts *immediately* upon the globe of the radiometer; under the other, those in which *light* and *heat* act indirectly upon it. The experiments will make these definitions more intelligible.

I. I brought radiometer A into a dark room (without a fire), and lighted a small benzine lamp, placing it almost directly over the top of the globe, and about two feet from it. I turned down the flame until the vanes were only just visible. The light had no effect on them, and in fact had none when I afterwards turned it up to its full height. I then placed one finger on the side of the globe, much *nearer to a black than to a white* disc (for convenience I shall call one side of a vane the white, the other the black disc). The black was at once repelled, but not with sufficient force to allow the white disc on the next vane to pass the heated spot, and so bring the next black disc under its power. I then allowed the globe to cool, and placed my finger as before, only much nearer to a white than to a black disc. The white was repelled nearly as much as the black had been, but the next vane was not brought past the heated spot. In no way could I force a vane through an angle of 90°. With two fingers, one on each side of the globe, the effects were the same as when I used one finger. I then surrounded the globe completely with my warm hands; no rotation was produced. The discs moved backwards and forwards through an angle of about 40°. I then allowed the globe to cool—no motion. Bringing radiometer B into the same position, I placed my finger as before, nearer to a white disc. It was repelled through an angle of about 40°, but the next vane was not brought past the heated spot. On removing my finger, the black disc gradually *approached* the heated spot, and, as soon as it had passed it, the movement quickened, and complete rotation ensued. This backward movement, apparently resulting from repulsion of the white discs, I shall hereafter call the "reverse motion." As soon as the discs became stationary, I placed my finger nearest to a black disc; it was at once repelled; the white approached, passed the warm spot, the next black was then repelled—thus continuous rotation was produced. I then removed my finger, and allowed the globe to cool. Friction soon overcame momentum, the vanes stopped, and "*reverse motion*" *immediately commenced.* With a finger on opposite sides of the globe, the same effects are produced, but with greater intensity. When perfectly still I turned the flame of the lamp down so low that I could not distinguish anything in the room, and then surrounded the globe with my warm hands. In about half a minute, suddenly turning up the flame, I found the vanes rotating rapidly (making 40 or 50 complete revolutions per minute), the black discs being repelled. Removing my hands, reverse motion ensued; and as soon as the vanes had become stationary, the temperature being equalised, I turned down the flame, so that there was only just enough light to enable me to see the vanes, and again placed my hands round the globe. The black discs were repelled, rotation quickened, reached its maximum, and then gradually decreased in velocity until the vanes were perfectly stationary. The little air inside having been raised to the same temperature as that of my hands, when they were removed rapid reverse action at once ensued. When the vanes again became stationary, I allowed two or three drops of sulphuric ether to fall upon the globe. The rapid evaporation, producing cold, immediately caused reverse motion, which continued for two or three minutes, when the vanes again became stationary, and the motion was reversed, the black discs being now repelled as the globe was receiving heat from the outside air. This experiment can be repeated for any number of times; the hotter the surrounding air, the greater the effect. It can be produced as well in the dark as in the light, and if a single drop of ether fall on the globe every four or five minutes, rotation—first in one direction, and then in the other—must continue until the temperature of the whole of the surrounding air be reduced to that degree of cold obtained by the rapid evaporation of the ether. Thus, with a perfect radiometer, unlimited supply of ether, and means of dropping it when required, motion may be produced which must practically be perpetual.

The next experiments were tried in daylight. I placed B in front of the fire for a few seconds. The black discs were repelled with such rapidity that I could not distinguish the vanes. On putting it quickly outside my window, where the thermometer stood three or four degrees below the freezing point, the rotation soon ceased, and reverse motion set in, at first with a velocity equal to thirty-three complete revolutions per minute; this motion, gradually decreasing, continued for nearly ten minutes. These and many other similar experiments seem to point to the conclusion that when a radiometer is cooler than its surroundings, or, in other words, is receiving heat, the black discs are repelled; but if, on the other hand, it be warmer than its surroundings, and is giving out heat, the white will be repelled, or the black attracted.

By covering the globe with hot and cold glass shades alternately, I have made the discs revolve in whichever direction I wish. I have no doubt that all radiometers with reflecting and absorbing discs may be made to rotate in either direction, provided they are properly treated, some requiring greater differences in temperature than others. I find that my A, in order to obtain reverse motion, must be subjected to a change of nearly 40°, while B requires very much less. The latter is so sensitive that, when rotating under the influence of sunlight, reverse motion sets in when the window-blind is drawn down. The effect of hot or cold breath upon the glass is similar to the above; so one can *blow* the vanes round in either direction.

When A and B were placed on the chimney-piece, both the black discs were repelled under the influence of diffused sunlight; A with a velocity equal to three revolutions per minute, and B seven. Both were then covered with equally cold glass shades (at about freezing point). In five seconds B stopped and reversed motion (five revolutions per minute), stopping in about $1\frac{1}{2}$ minute, and then again repelling the black. A simply decreased in velocity for a minute or two, and then moved as quickly as before being covered with the shade. When a candle was placed seven inches from B, the black discs were repelled at the rate of eleven revolutions per minute. I then covered it with a cold glass shade. Motion ceased in thirty seconds, and reverse motion set in (against the candle-light), and continued for forty-five seconds, when it again stopped, and original motion ensued. I then placed A $10\frac{1}{2}$ inches from the same candle, which caused the black to be repelled with the same velocity (eleven revolutions per minute). When covered with the same glass shade, re-chilled to the same temperature, the rotation decreased to six revolutions per minute, and then gradually quickened as the shade became warmer. To cause the black to be repelled six revolutions per minute, I had to place the candle $15\frac{1}{2}$ inches from the globe.

I must now pass on to section II. The last-mentioned experiment comes partly under one section and partly under the other.

When a candle or any source of light is placed, say, 6 inches from the globe, the heat, having passed through 6 inches of air, is incapable of heating the residuum of air within the globe, and unless the radiant heat is very great, the glass is not appreciably warmed. I placed one candle about 12 inches from B, and found its light (or heat) capable of producing slow black repulsion. Eleven other candles were then lighted on the same side of the globe, and at the same distance from it; they produced quick rotation. Ten of them were then suddenly blown out; the vanes soon became stationary, and reverse motion set in, the black discs moving against the light of the two candles. Under other conditions, these two candles would have caused them to be repelled with a velocity equal to $1\frac{3}{4}$ revolution per minute. With three lighted candles, and then two of them blown out, the same effect was produced, though not with the same intensity. By raising or lowering the flame of a benzine lamp, rotation in either direction can be produced; the degree of reverse motion, I find, always depending on the degree of diminution of light. These effects cannot be produced with radiometer A.

Into a large glass jar, filled with a strong solution of alum, I placed A, the globe being surrounded with nearly three inches of the solution. When the solution was of the same temperature as the air of the room, I placed a lighted candle one inch from the jar, another exactly opposite, one on either side, and so increased the number to nine, but no movement of the discs was caused, except when the sixth candle was lighted. Then the discs moved through an angle of 135°, and again became stationary. The reason of this will presently be explained. When the tenth candle was lighted, the black discs were repelled with a velocity of $2\frac{1}{2}$ complete revolutions per minute; with the eleventh candle, $3\frac{1}{4}$; twelfth, 4; thirteenth, $4\frac{1}{2}$; fourteenth, 5; fifteenth, $6\frac{1}{4}$; sixteenth, $8\frac{1}{2}$. By gently stirring the top of the solution with a glass rod the movement was increased to $10\frac{3}{4}$ revolutions per minute. I find the action of light which has passed through alum solution much greater when the solution is not perfectly still. When the sixth candle was lighted an accidental shake given to the table produced a little motion in the liquid, and the discs moved as mentioned above. If the solution be stirred too violently, the colder liquid from the bottom is forced upwards, and rotation ceases. This can also be effected by allowing a little of the solution, chilled in a pipette, to run down the side of the globe. By the same means, a little of the solution when heated will produce repulsion of either disc, as in the experiment with a warm finger, already mentioned under section I.

I tried on B the effect of light after it had passed through different thicknesses of alum solution. The repulsion of the black was always much decreased, varying with the thickness used. When the light of three candles passed through half an inch of the solution, slow rotation was produced. Two were blown out; the vanes stopped, and reverse motion ensued. The two candles were again lighted, and a short piece of magnesium ribbon burnt behind them. Rotation immediately quickened, and as soon as the ribbon came to an end, the vanes stopped, and reverse motion ensued. The same effects were produced when a thickness of 8 inches of the solution was used, but in a less degree. I find that whenever light, passing through an alum solution of whatever thickness, produces black repulsion, reverse motion begins as soon as the source of light is removed or in any way diminished. The more perfect the radiometer, the less this diminution need be. In a radiometer with bad reflecting surfaces, the black repulsion will only decrease in velocity. Motion may altogether cease if the degree of diminution of light be very great, but reverse motion cannot be produced. I find that the light of three candles, when passed through half an inch of the alum solution, and causing slow black repulsion in the radiometer, is capable of slightly heating the *blackened* globe of a differential thermometer, and causing a depression of the sulphuric acid equal to about one-thirtieth of an inch, the other globe being carefully sheltered from the light. I could not observe any depression when a greater thickness of alum solution was used.

I find that a source of light, however bright, cannot produce rotation of the vanes of a radiometer unless it is capable of raising the temperature of the residual air within the globe. Two or three experiments, I think, prove this. I placed B close to a given source of bright light; very rapid black repulsion at once ensued. Taking it from its stand, and heating the whole of the globe and stem over the flame of a spirit lamp until the glass was very hot, and the air inside raised to a like temperature, I quickly placed it in its original position, and found that the light had no effect, the vanes remaining perfectly stationary for more than a minute ; when, the globe and internal air becoming cooler, black repulsion ensued. Wishing to produce this effect without sensibly heating the globe, I placed the stem of B in a large test tube filled with cold water. The bright light of a candle placed 3 inches from the globe produced quick rotation. I then heated the test tube with the flame from a spirit lamp, taking care that the hot external air should not ascend and warm the globe. As the water became warm, and heated the air in the stem of the globe, the hot internal air rose, the black repulsion became less and less, and when the water nearly boiled all motion ceased for a few seconds, and *reverse motion set in.* The air inside now being warmer than the candle flame could make it, gave out heat instead of receiving it. I then tried the converse experiment. After counting the number of "black repelling" revolutions produced in one minute by the candle when the radiometer had obtained equal temperature with the surrounding air, I chilled the air in the top of the globe with "ether spray." The heat the internal air received from the black discs was thus quickly disposed of, and the candle was able to produce greater effect, which was apparent by the increase in the velocity of the rotation.

Wishing to somewhat vary this experiment, I fastened cotton wool tightly round A, until it was encased in a thickness of nearly an inch, leaving a part of the globe uncovered about the size of a half-crown piece, thus preventing to a great extent any radiation of heat from any part of the radiometer. I then covered it with a large glass jar, and on one side placed a solution of alum about half an inch thick, which prevented the radiant heat from nine candles warming the globe. These nine candles were thus placed about 16 inches from the radiometer; their light having to pass through the alum solution, the two glass sides of the vessel containing it, the glass jar and the glass of the globe, before it could affect the discs. They were of such a height that the more they burnt away the greater effect they ought to produce, as the more light could then pass through the different glasses and the solution, and through the opening in the cotton wool placed nearly opposite to them. This opening was so situated that the light should fall almost entirely on the *black* discs. Soon after the nine candles were lighted I noticed that they caused the black discs to be repelled with a velocity equal to $4\frac{1}{4}$ complete revolutions per minute. After burning for 15 minutes, the velocity had reduced to $3\frac{1}{4}$ revolutions, and in time, I have no doubt, would have almost ceased. Then suddenly taking off the cotton wool, the velocity increased to $5\frac{1}{8}$ revolutions, at which rate it remained without change. Four candles produced at first $2\frac{5}{8}$ revolutions, decreasing in 15 minutes to $1\frac{1}{2}$, and when the wool was removed immediately quickened to nearly 3. The great difficulty in these experiments is to prevent the loss of heat from the globe by radiation. If all is preserved, rotation, sooner or later, would probably cease.

I have reason to believe that moonlight is capable of repelling the black discs, and that without the use of a condensing lens or telescope. I find the experiment attended by many difficulties; but if the radiant heat from the observer's body at a distance of 8 feet from the radiometer is incapable of affecting the discs, when the temperature of the surrounding air is 10° below freezing point, then moonlight will cause the black discs to be slowly repelled at a rate of about one complete revolution in two minutes. This was tried with my radiometer B. With A the velocity would be greater. According to the theory which has been suggested to me, the repulsion or attraction of the black discs is caused by the direct action of heat, and it seems to be able to account for all the phenomena I have as yet observed. The way in which it affects the discs is somewhat similar to that shown in the "Trevelyan rocker." Let us suppose a radiometer with good reflecting and absorbing discs to be at rest, a candle to be then placed at a distance of 6 inches from it; the light or heat from the flame, having crossed 5 inches of common air, is incapable of heating the residuum of air within the globe. It is arrested and absorbed by a black surface, which becomes warm, and heats the adjacent particles of air. They expand, and give the disc a push into the cooler air behind, which the reflecting, non-radiating surface was incapable of warming. The ordinary black repelling motion would be thus produced; the greater the heat the more the expansion, and the quicker the repulsion. Remove the source of heat, and chill the globe. The friction soon overcomes the momentum, and the discs are at rest; the air inside is warm; the black absorbs a little of the heat close to its surface; causes contraction of the air. A space is formed into which the disc is pushed by the hot air behind it—the backward or "reverse motion" would thus be caused.

This reverse motion can never be very great, as the (motive) heat is limited in quantity, and it is expended by gradual conversion into mechanical action, which has friction to overcome, and also is lost by radiation from the outside surface of the globe. It is evident that unless the reflecting surfaces are good, this backward motion could not be obtained, as both sides of the vanes would cool the adjacent air by absorption, and the difference between the two actions would not be sufficient to cause the rotation. This is why I can get little, if any, reverse motion in my radiometer A.

When heat is applied to the globe direct, as is done by touching it with the finger, &c., the action is somewhat different. The glass becomes warm, and it warms the air within, producing expansion, which repels whichever side of a vane is nearest to the warmest part of the globe. If this happens to be the white side, no rotation can be produced, because the continued application of heat repels the white, and the opposite black disc (on the next approaching vane) absorbs the heat, and has, as in the first case, a strong tendency to move in the opposite direction; but should it happen that a black disc is nearest to the warm glass, rotation immediately ensues. The black being repelled by the expanded air, and taking with it into a cooler part of the globe enough heat (obtained by its absorptive power) to enable it to be further repelled, as in the way already described, the next black disc is thus brought under the influence of the warm glass, and the operation is repeated. If, when the white disc be first repelled, and no rotation produced, the finger be removed, and the globe chilled, the black gradually approaches the heated part of the globe, absorbing and radiating the heat, and so contracting the air, and making room for itself, as mentioned in the description of "reverse motion." I noticed that the black surfaces on my radiometer B were gradually becoming more *grey* every day; in fact, the black (lamp black, I believe) was coming off little by little—in places it had worn almost white. This was not the case with A, although it had done much more work. I at once thought that this was caused by the reverse motion, the black particles having been *sucked off* by the contracting air. I therefore placed B on its side, and so collected the black dust in one spot in the bottom of the stem of the globe, and carefully placed it upright again. I then heated it, and so caused the black discs to be repelled with great velocity, carefully noticing at the time, by the help of a magnifying lens, whether any black particles fell on to that side of the stem that I had cleared of them. Not one appeared until the reverse motion began. I was then soon able to count a score or more. I repeated this experiment two or three times, and always with the same effect. It must not be tried too often, as every time it makes the black discs less sensitive to the action of heat and light for further experiments.

I think one can compare the residuum of air within a radiometer to a piece of elastic stretched almost to its utmost, but far too strong to be *broken* by any "Sprengel air-pump."

I have sometimes used the word "heat," and sometimes "light" in describing these experiments. They cannot be considered as two separate agencies. All that is necessary to produce "black repulsion" in a radiometer is that one should use some part of the spectrum between the ultra red or heat rays, wherever they may begin, and the extreme luminous part. The action decreases rapidly from the red towards the blue. If the *ultra* red (or "*active* heat") rays be cut off by the intervention of an alum solution, the red rays (the others giving but very little help) are absorbed by the black surfaces, and a little heat is produced, which it seems causes the repulsion. If a source of light be used which is very deficient in red rays, the light, however intense, is hardly capable of moving the discs. I have used the electric spark from a "6-inch" induction coil, intensified by introducing Leyden jars into the secondary circuit, and allowed the continuous stream of sparks to pass between platinum electrodes. This dazzling light, when concentrated by a condensing lens on to a black disc, had no effect whatever, although allowed to remain there for many minutes. The same spark, when passing between carbon electrodes, repelled the black, and produced *very* slow rotation. The carbon was made warm by the electric current, and the radiant heat doubtless produced the effect.

From the experiments I have described, I think we can deduce three leading principles which govern the movements of radiometers having *good* reflecting and absorbing discs.

I. When a radiometer is receiving light or heat, being at a lower temperature than its surroundings, repulsion of the black discs must ensue, and continue until the temperatures are equalised.

II. When a radiometer is radiating heat, being at a higher temperature than its surroundings, attraction of the black discs or the apparent repulsion of the white discs, must ensue, and continue until the temperatures are again equalised.

III. No source of light can produce repulsion of the black discs of a radiometer unless it is capable of raising the temperature of the residual air within the globe.

RAILWAY TRAVELLING AND ELECTRICITY.

By W. PREECE, C.E.,

OF THE POST OFFICE TELEGRAPH DEPARTMENT.

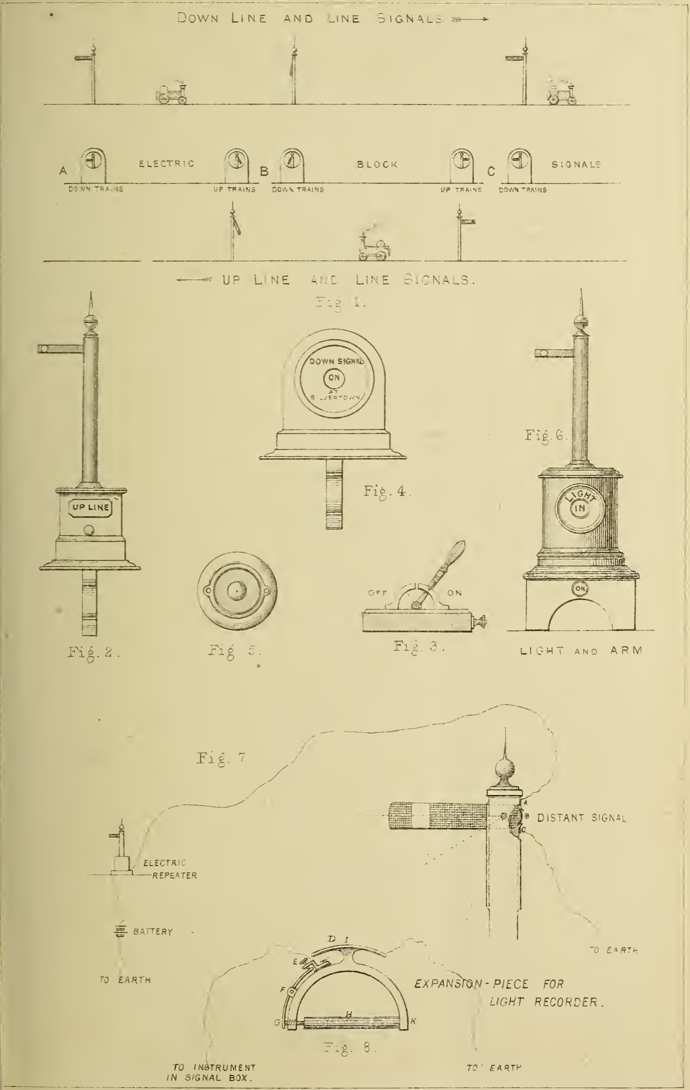
PLATE CXXXIV.

TERRIBLE railway accidents like that at Abbot's Ripton excite the public interest with great intensity for periods of time varying with the magnitude of the disaster, and the amount of other stirring events occurring simultaneously. There is no sensation greater than that of a severe railway accident. It affects every one. We are all travellers by railway, and self-interest makes us read with horror and dismay of the death of *units* in a railway train, while we pursue our breakfast with comparative calmness during the recital of *hundreds* smothered to death in a colliery explosion, or sent to eternity in a watery grave. The sensation is aggravated by the public press, which gives such prominence to every detail of what is called "this railway butchery," while it leaves unchronicled the terrible slaughter that occurs daily in our midst from other causes. There are more people killed *every day* by accident than are killed *every year* from causes beyond their own control on the railways of the United Kingdom! Mr. John Bright said that a first-class carriage was the safest place to deposit oneself in, and a perusal of the following statistics will go far to support that opinion.

In 1873 no less than 17,246 persons met with violent deaths in England and Wales, which is an average of 750 per million, or 1 in 1,354, or 47 per day. The causes of these deaths are thus analysed :—

TABLE I.—VIOLENT DEATHS IN ENGLAND AND WALES FOR THE YEAR 1873.

Cause of Death.	No.
Injuries in mines	990
Mechanical injuries (not on railways or in mines)	6,070
Chemical injuries	2,784
Asphyxia	5,193
Violence (unclassified)	919
Railways	1,290



Improved Railway signals.

Some of these may be further analysed as follows:

TABLE II.—ANALYSIS OF TABLE I.

Cause of Death.	No.
Mechanical Injuries—	
Fall from scaffold (ladder)	165
„ „ window	70
„ downstairs	456
„ in ships and boats	134
„ from height	500
„ in walking	93
„ (not stated how)	530
„ of heavy substances on	509
Horse or other animals	269
„ conveyance	1,250
Machinery	1,132
Fight	5
Blow, &c.	124
Gunshot wounds	185
Chemical Injuries—	
Burns	1,064
Scalds	701
„ (drinking hot water)	50
Lightning	21
Sunstroke	96
Exposure to cold	138
Asphyxia—	
Drowned	3,232
Suffocated by food	94
„ bedclothes	611
Hanged, strangled, and executed	581
Murder, manslaughter, and suicide	228

Now let us take accidents to railway passengers from causes within and beyond their own control:

TABLE III.—ACCIDENTS TO RAILWAY PASSENGERS, FROM CAUSES WITHIN AND BEYOND THEIR OWN CONTROL.

Date.	Within own control.	Beyond own control.	Total.
1871	45	12	57
1872	127	24	151
1873	120	40	160
1874	125	86	211
Average	104	41	145

This is an average of forty-one persons killed annually from causes beyond their own control; and it shows, in fact, that the railway companies are in reality more mindful of the lives of their passengers than the passengers are of their own lives.

These latter accidents can be classified as follows:

TABLE IV.—ACCIDENTS TO RAILWAY PASSENGERS IN 1874, FROM CAUSES WITHIN THEIR OWN CONTROL.

Cause of Accident.	No.
From falling between carriages and platforms	49
Getting out of or into trains in motion	22
Crossing the line at stations	33
Falling down stairs at stations	2
Falling out of carriages during travelling of trains	9
Other accidents	10
Total	125

This, however, is not the death-roll from all causes on all railways of the United Kingdom during the year 1874. The total number of persons recorded at the Board of Trade as having been killed was 1,424. Of these 211 were passengers, and, of the remainder, 788 were officers or servants of the railway companies, or of contractors, and 425 were trespassers, or suicides, or others who met with accidents at level crossings or from miscellaneous causes.

1874 was, however, a very exceptional year, for no less than 71 passengers were killed in the three fearful accidents on the Great Western at Shipton, on the Great Eastern at Thorpe, and on the North British at Bowness Junction.

We cannot contemplate these figures without feeling that, compared with other causes, the danger of railway travelling must be exaggerated. That there is danger in a train no one can deny. It is impossible to stand upon a platform when an express rushes by without feeling that there is but a rivet, a bolt, or a rod between life and death. Yet, though we have just read the harrowing accidents of a dreadful collision in the north, we unhesitatingly entrust our precious bodies in a railway carriage to the south, is a proof that we have faith in our railway management, and that we tacitly admit that there is also safety in a train. Our ideas of safety are but relative. If the press gave as much prominence to the 94 cases of choking, or to the 456 cases of falling down stairs, or to the 611 cases of being suffocated in bed, as it does to the railway accidents, we should hear less of the railway "juggernaut," and more of the skill and care with which the enormous railway traffic of the country is conducted.

The examination of the interesting statistics compiled by the Board of Trade shows that, taking the following periods, the proportion of passengers killed from causes beyond their own control to passenger journeys made was:

TABLE V.—PROPORTION OF PASSENGERS KILLED TO JOURNEYS MADE.

3 years ending 1849	. .	1 in 4,782,188 journeys made.
4 " 1859	. .	1 " 8,708,411 "
4 " 1869	. .	1 " 12,941,170 "
3 " 1873	. .	1 " 20,089,660 "

This gives striking evidence of steady and constant improvement in the conduct of the railway traffic, for the safety in the period ending 1873 was nearly five times greater than that in the period ending 1849.

If we take the average length of each journey at 10 miles, one passenger is killed, from causes beyond his own control, for every 200,896,000 miles travelled. Thus, if a person travelled 10 hours a day at the rate of 30 miles an hour for each of the 365 days of the year he would probably be killed in 1,835 years.

How has the potentiality of danger of railway travelling been converted into comparative actuality of safety? Freedom from accident depends upon the perfection of the road, of the rolling stock, of the signals, and, above all, of the men. But none of these elements are perfect. Accidents have been analysed into—

Defective permanent way . .	18 per cent.
, rolling-stock . . .	13 "
" signals	28 "
" human machinery . .	41 "

Classified for the period 1870–74, they show the result given in the table on next page.

Zeal and anxiety, the necessary evils of a state of tension due to increasing traffic; want of punctuality; late arrivals of the public; and variable weather, become an absolute source of danger. Every accident is traceable to its cause. Purely inexplicable accidents are unknown. Hence, though considerable improvements in the mode of working have been made—as are indicated in the continued progressive increase shown in ratio of killed to journeys made in Table V.—further improvements are certain. But all improvements bring their own evils, and the greatest of these is human fallibility. The body will tire, and the brain will get out of gear. Pure wilfulness, carelessness, or mischief are extremely rare. Who does not

Nature of Accident.	1870.	1871.	1872.	1873.	1874.
From engines or vehicles meeting with, or leaving the rails in consequence of, obstructions, or from defects in connection with the permanent way or works . .	9	19	21	24	18
From boiler explosions, failures of axles, wheels, tyres, or from other defects in the rolling stock	10	22	17	23	13
From trains entering stations at too great speed	—	2	7	5	—
From collisions between engines and trains following one another on the same line of rails, excepting at junctions, stations, or sidings	61	9	22	18	9
From collisions at junctions . . .	18	19	32	20	22
From collisions within fixed signals at stations or sidings, &c.	Included in the above 61.	63	91	98	75
From collisions between trains, &c., meeting in opposite directions	3	2	5	3	6
From collisions at level crossings of two railways	1	—	—	3	1
From passenger trains being wrongly run or turned into sidings, or otherwise through facing points	14	12	34	36	17
On inclines	6	11	9	11	7
Miscellaneous	9	12	8	6	—
	131	171	246	247	168

make a mistake? In the year 1874, 4,400,000 letters out of 967,000,000, or one in 220, found their way to the Returned Letter Office. 89,540 undelivered letters contained valuables, and bank-notes, bills, &c., the value of which alone amounted to 565,000*l.*; 337 of these had no addresses; 61,000 postage stamps were found loose in the different post-offices, and 20,000 letters were posted without any address at all.

How then is the comparative safety of railway travelling produced? By taking advantage of the lessons taught by experience, and by applying the means suggested by scientific thought and inventive skill to remedy defects. Failure has thus led to improvement. Every accident has been a lesson learnt, and bitterly have those suffered who have not profited by such writings on the wall. The particulars evidenced by each accident have been carefully and systematically recorded in the reports of the inspecting officers of the Board of Trade, and thus by recording past experience the materials are collected for carefully generalizing the laws of railway working and for establishing a true science of steam locomotion.

Telegraphy, or the art of conveying information by certain preconcerted signals to the ear and to the eye, is the chief aid of the railway engineer. Thus, at every railway station, level crossing, or junction, signal-posts are erected which convey to the approaching engine-driver—by exposing discs, bars, or semaphore arms in different positions by day, or lamps displaying different colours by night—the fact that the line is clear for him to proceed or obstructed so that he must stop. The favourite signal by day—the survival of the fittest—is the arm, which, when at right angles, implies *danger*, and when at an angle of 45°, *safety*, and

> White means right: red means wrong:
> Green means slowly go along,

teaches the young railway lad the rule of the road by night. The character of every train is indicated by its *head lights*, and its presence to an approaching train by its *tail lamps*. Should thick weather prevent the sight of the signals, detonating fog signals announce the contiguity of danger. The marshalling of trains in station yards and platforms are produced by whistles and flags by day and lamps by night, all forming a species of telegraphic language between the fixed station and the moving train.

Where telegraphy is required to reach distances beyond the sphere of the ear or the eye, electricity is employed, and the electric telegraph becomes of prime and essential use, not only in regulating the traffic on double and single lines, but in securing safety. Special trains are moved about by its means, delays are remedied, breaksdown rendered harmless, runaway engines have been overtaken by its aid, passengers' luggage recovered; but, above all, irregularities are by its means rapidly announced, and the evils of unpunctuality rendered innocuous.

The greatest element of safety on railways is, however, the Block System.

The block system arose out of the multiplication of trains, and the necessity for increased speed. Necessity, the mother of invention, brought it into existence.

By it trains travelling upon the same line of rails are kept apart by a certain and invariable interval of *space*, instead of by an uncertain and variable interval of *time*.

The practice under the "time" system is to exhibit the danger signal for five minutes, and the caution signal for five minutes more, after a train or engine has been despatched from or past any station, junction, level crossing, or siding. Trains are thus said to be kept apart by fixed periods of five minutes, and if the caution signals were properly regarded, by an interval of time even longer than that. The safety of the train is entirely the responsibility of the driver. Immunity from accident is dependent upon his keeping a clear look-out. If engines ran at regular and fixed speeds, if time-tables could be adhered to, if the line were not crowded with traffic, if the driver could always ensure a good view before him, if signals were near together and they were properly regarded, then a rigid interval of time might be maintained between following trains; but none of these elements of safety are constant. Fast expresses follow slow goods trains, now through a thick fog, now up a wet incline, at one moment in bright sunshine, at the next in a thick snowstorm; creeping mineral trains break down in a long interval between two stations; passengers rush in at the last minute, detain the train, and prevent the time-tables from being adhered to; trains are so frequent at some places that the five minutes' interval cannot be adhered to; obstructions to view arise from curves or cuttings, or from atmospheric causes; long lengths of line are unprotected by any signal at all, and signals themselves are too frequently neglected. Hence the system is brimfull of elements of danger, and the inexorable logic of facts has shown that the "time" interval is illusory and the system unsafe.

But when trains, however rapidly or slowly they may be running, however much punctuality has been infringed, however crowded with traffic the line may be, are invariably kept apart by an interval of one or two miles, collision between them becomes impossible. This is the *block system*, which has, very improperly, been divided into two classes, the *absolute* and the *permissive*. The former is the block system proper, the latter is not a "block" system at all, but a system introduced, not to secure the safety of trains, but to increase the capacity of the line for the transmission of increasing traffic. It is, doubtless, an improvement on the time system, but it bears little affinity to the block, and should certainly not be included in the same category.

The block system is effectually carried out by means of electricity.

The line is divided into sections, each having its own instruments for up and down trains respectively. Let A, B, C (fig. 1, Pl. CXXXIV.) represent three signal stations, and the space between them two sections of the line. Let the upper line be that for down trains, the lower that for up trains, and the centre the block signals. The connection between the latter exists only from one signal station to the other. Each line of rails has its own instrument, and each instrument its corresponding line, or out-door, signal. The block signals are for the guidance of the signalman; the out-door signals for the guidance of the engine-driver.

The electric signals consist of a miniature semaphore signal (fig. 2) capable of giving two signals—one, the arm raised to a horizontal position, indicating *danger*, or stop; the other, the arm lowered or depressed, indicating *all clear*, go on—for each line of rails; a switch (fig. 3) by which the electric semaphore is worked, a bell (fig. 4), and a bell-key (fig. 5).

The semaphore is the *block* signal. The object of the bell is to indicate by varying numbers of beats the nature of the signal sent or about to be sent. Thus we have for the "warning signal," "train coming—look out," twice two beats—*i. e.* two beats followed by two beats, with a sufficient pause between to indicate it is not meant for four consecutive beats. Two beats for the "departure" signal, three beats for the "all clear" signal, six beats for the "obstruction" signal, and so on.

Let us now take diagram No. 1 and follow out the signalling of an up and down train. Say we have a train about to start from the terminal station A in the direction of B. It is a down train. The warning signal is first sent, and acknowledged by B. When the train is ready it is started, and its departure is notified. The warning signal was twice two beats sounded on B's bell; the departure signal two beats only. B now knows the train has left A, and it is his duty to prevent another following till the first has arrived at B. To do this he merely places his electric switch (fig. 3) at ON, which raises the electric arm for the down line at A to *danger*. The signalman at A has already, on the departure of the train, at the moment when he signalled its departure to B, placed his out-door signal for the down line at danger, and it has to be retained in that position till the electric arm is again depressed by B. No other train can now, without disregarding the signals, leave A for B. At this moment the position of the signals is that represented for down trains in the diagram (No. 1) between these two stations. The train is running through the section, and is protected in its rear at A.

B, on receiving the departure signal, has warned C that a train is coming by signalling to him the twice two beats upon his bell, in the same manner as did A to B. We will now assume that the train has reached B. It does its work at the station, if it is a station, sets down passengers, takes up others, and now it starts towards C, the electric arm for that section standing at "all clear." Its departure is announced, and C raises the electric arm at B against any following down train. The out-door signal for that line has been placed at danger, and now that B is perfectly clear of the train he is at liberty to tell A he can send on another train. He does so by placing his switch-handle (fig. 3) this time to OFF, which lowers the electric arm at B to the "all clear" position, and rings the bell three times. The signalman at A then lowers his out-door signal to the same position, and all is ready for a train to follow.

Up trains are dealt with in a precisely similar manner. We have an up train running betwen C and B. At C we observe the electric signal for up trains shows danger, and the out-door signal the same; the signals in advance are at "all clear." When the train has arrived at B, done its work and departed, the section B C will be cleared, and C will be able to send on another train.

On single lines, that is lines which have only one set of rails on which both up and down trains are worked, additional precautions are taken. Before the train starts from any signal station the section is blocked against any train approaching in an opposite direction. It then leaves, and the station in advance of it blocks it in the rear. It is thus protected in advance and behind, section by section, throughout the line.

The operation of signalling, although somewhat tedious in description, occupies but a very little time; the pressure of a bell-key and the touch of the switch-handle are all that is required. The instruments are arranged in the most convenient manner over the signalman's frame, so that he can even carry on his electric signalling by one hand whilst with the other he operates the levers working the out-door signals. So rapidly is all this performed that trains frequently follow over busy sections of line within two and three minutes of each other. The sections are of course regulated to the traffic. Where it is dense they are necessarily short, the limit being the distance sufficient to enable a train to pull up before reaching the next signal station. Thus there are, in places, sections as short as half-a-mile, whilst in others they are five miles in extent.

But apart from the protection which electricity imparts to railway travelling, and the facility it offers for adjusting and regulating the traffic, there are innumerable purposes for which the telegraph is employed to facilitate business and to secure efficiency. The distribution of correct time, the collection of spare trucks and coaches, the relief of staff, the supply of assistance in cases of accident and danger, and—not least—the reparation of the errors and thoughtlessness of passengers.

It is used on some lines to establish an effective means of communication between passenger and guard; and perhaps one of its most useful applications is to record in the signal box, before the signalman's eyes, the position of the signal-arm by day and the condition of the light by night, which is hidden from his sight by the formation of the line, buildings, darkness, fog, or steam. Electric repeaters are one of the greatest elements of safety in working railways. Fig. 6 represents an arm and light repeater combined. A battery is placed in the signal box (fig. 7), one pole of which is to earth, the other is connected to the instrument, from which a wire is carried to the signal post, where it is joined to a spring, A, in close proximity to the back of the arm to be repeated. To the back part of the arm itself is fixed a piece of metal, B, and in contact with it a spring, C. When the arm is raised *fully* to the *danger* position, the spring A is free from the plate B, but the slightest depression of the arm forms a connection between them, and so completes the electric circuit. The arm of the repeater is maintained at danger by gravity, and is lowered by the electric current. This arrangement can of course be reversed, *i.e.* the repeater arm may be raised by the current and lowered by gravity if required, or the movement of the arm in its several stages of danger, caution, and clear, can be recorded with the same wire.

The record of the light requires a separate and independent wire and battery. One pole of a battery is carried to the instrument, the other being put to earth, and a wire is then carried from the instrument to the insulated stud D (fig. 8) of an iron frame, I, fixed within the lamp at the signal post, in such a manner that the hollow tube, H, shall stand immediately over the flame. This tube, H, is firmly fixed to the iron frame at K, but is free to move at, and in the direction of, G. G F is a small metal lever centred at F, kept by means of a spring in contact with the adjusting screw, E. To the frame I is attached a wire connected with the earth. If, now, a flame be brought beneath the tube H, the heat from it will cause the metal to expand. In expanding, it will move in the direction of G, press against that end of the lever G F, and so carry its opposite extremity free of the adjusting and contact screw, E. In other words, it will break the electric circuit at E. The recording instrument is provided with an indicator having on it the words OUT and IN, which is so weighted, or adjusted, that when not under the influence of an electric current, the word IN shall be brought up to the aperture in front of the instrument, making it then read, as in the engraving, LIGHT IN. This, then, is what happens. The battery is always in circuit ready for action. So long as the light is burning, the tube H is expanded and breaks down the circuit. The moment the light goes out or gets dim, the tube again contracts, and the electric circuit being restored, the current flows through the instrument, carrying over the indicator so as to exhibit the word OUT instead of IN, and at the same time sets in motion a bell which continues to ring until the light has attention, or until the bell is turned out of circuit by means of the switch (ON) in front of the instrument.

The operation of scientific thought has introduced many mechanical elements of safety into railway working, which are as ingenious as they are effective. Improved permanent way, the interlocking of signals and points; the concentration of levers in well-constructed cabins; effective brake power; perfect tyre fastenings; better coupling arrangements, and superior engine and rolling stock, have all aided to secure that simplicity of working and safety in travelling which undoubtedly exist.

But, as the principal element of danger in railway travelling consists in the fallibility of the human machine, it must not be forgotten that we owe our immunity from accident as much to the careful selection, education, and supervision of the staff and the maintenance of good discipline, as to the appliances of scientific skill. Science cannot be devoted to a nobler purpose than to the protection of human life, and the records of experience show that it has earned well-deserved laurels in rendering the dangers of railway travelling potential and its safety actual.

STUDIES OF MATTER AND LIFE.

By HENRY J. SLACK, F.G.S., Sec. R.M.S.

THE discoveries of recent science have greatly affected the notions we are able to form concerning the relations of force and matter, and likewise of the connection between physical agencies and manifestations of life. In studies of this description we are struck with the amount of force that lies potentially in extremely minute quantities of matter ready for vigorous action the moment the right stimulus is applied, and by the way in which quickness of motion makes up for smallness of weight. The physical inquirer is not obliged to tarry for the curious and important investigations of the metaphysician; he need not attempt to settle the fundamental questions—what is matter? and what is force?—in the ultimate constitution of either. To the experimentalist, matter is known by what it does; and whether the problem before him relate to mechanics, chemistry, electricity, light, heat, or gravitation, it is with matter in motion and exhibiting force, because in motion, that he has to deal. The same may be said of all the physical processes and manifestations of life, though we seem no nearer than the ancient Greeks were when we try to understand the connection between motions of particles and the phenomena of feeling and thought.

Light, heat, electricity in its various forms, chemical force and nerve force, are not only now classified as "modes of motion," but the motion in each manifestation of these forces appears to be wave motion; and it is probable that gravitation, the correlation of which with other forces is not yet established, may at last be found to be a wave motion also. In wave motion each particle moves, pendulum-wise, backwards and forwards in a small curve, transmitting the motion to the particles before it in more or less rapid succession, the motion becoming weaker as the original impulse is divided amongst more and more particles, until at last it is so enfeebled that it cannot be observed. The common illustration of throwing a stone in a still pond and watching how, as the outward circling wavelets spread, they grow weaker and weaker, and if the space is large enough, at last seem lost in the calm beyond—this affords a good notion to begin with of wave forms and wave force; but suppose, instead of a stone striking the surface of water, a sudden explosion took place of a particle of dynamite at some depth below. Here we should have waves in all directions, ascending, descending, and spreading on every side. Such waves would bear somewhat the relation to the pond-waves that a well-known toy composed of balls within balls would do to a mere section of the whole concern. Wave beyond wave in consecutive series, spreading in all directions from a point, must be conceived as spherical shells one outside the other like the coats of an onion, each expanding and contracting within narrower limits, and sending the wave force and the wave form onwards to an indefinite extent.

The quantities of matter acted upon by these wave forces may be very small, and yet the power exerted very great. Thus "Faraday found the quantity of electricity disengaged by the decomposition of a single grain of water in a voltaic cell to be equal to that liberated in 800,000 discharges of the great Leyden battery of the Royal Institution. This, if concentrated in a single discharge, would be equal to a flash of lightning. He also estimated the quantity of electricity liberated by the chemical action of a single grain of water on four grains of zinc to be equal in quantity to that of a powerful thunderstorm." * Tyndall himself also beautifully illustrates this subject in his remark: "I have seen snow-flakes descending so softly as not to hurt the fragile spangles of which they were composed; yet to produce from aqueous vapour a quantity which a child could carry of that tender material, demands an exertion of energy competent to gather up the shattered blocks of the largest stone avalanche I have ever seen, and pitch them twice the height from which they fell."†

When galvanic electricity is employed to decompose water, the constituent elements, oxygen and hydrogen, are not merely *allowed* to separate, but they are pulled apart with great force, and Gassiot showed that if the water were confined in iron bottles an inch thick, a small battery gave force enough to split them asunder. The directive force which the particles of water obey in the act of freezing, and which leads to its expansion, has similar power; and, as is well known, a very small quantity of water will burst a strong shell or split a great rock.

Although heat sets the molecules of all sorts of matter in

* Tyndall, "Notes on Electricity," p. 15.

† "Heat as a Mode of Motion," 4th edit. p. 147.

motion, not purely wave motion, it comes to us in a wave motion from the sun, conveyed like light by a material so attenuated as almost to reach the supposed condition of spiritual existence. This ether of space, which we can neither see nor feel, approximates to the conception, if such can be formed, of an immaterial substance. It must be so thin, and so light, that an inconceivable quantity would be required to weigh a pound, and yet when in motion the marvellous speed of its oscillations enables it to exert gigantic force. It can act so mildly that we are utterly unconscious that any substance strikes our eye when we see, or have a sensation of, violet light, in consequence of 700 million millions of its minute waves dashing against it in a second. But light can also cause chlorine and hydrogen to rush together with enormous force, and it can instantly tear to pieces chemical compounds held together by forces equivalent to prodigious mechanical powers.

Professor Josiah Cooke reckons that if this ether (of whose existence he is not quite satisfied) were as dense as common air, it would resist pressure on each square inch of seventeen million million pounds, just as air balances one of about 15 lbs. without suffering compression. He also tells us that if we could confine ether in a cylindrical vessel of sufficient strength to bear the pressure, and put upon it "a cubic mile of granite rock, it would only condense the ether to about the same density as that of the atmosphere at the surface of the earth." *

In consequence of its wonderful elasticity, ether can convey light waves about a million times quicker than air can convey sound waves, and some of the pulsations that reach us from the sun, and which lie beyond the violet end of the visible spectrum, must make their extremely short wave oscillations much quicker than the 700 millions of millions of violet light. Should, contrary to probabilities, the theory that space is filled with ether, and that ether has the properties mentioned, be ultimately found untenable, the measures of wave lengths and wave velocities must still refer to something positively existing. Light comes to us from the sun at the rate of about 190† millions of miles in a second, whatever it is; and when physicists say a wave of red light is about $\frac{1}{39,000}$ of an inch long, and a violet one about $\frac{1}{57,500}$ long, no doubter of the existence of ether, like Professor Cooke, hesitates to assume that they are quantities of

* "The New Chemistry," p. 23.

† The exact distance can never be known, as some residual error is unavoidable. When all the Transit of Venus calculations are finished and compared with the experimental methods adopted in Paris, the average result will probably be not far from 190 millions.

something. The probabilities are, however, enormously in favour of the theory that ascribes certain properties to ether, and that light and heat consist in its undulations. All known facts coincide absolutely with this theory, and it has been the means of leading physicists and mathematicians to fresh discoveries.

What is this ether, that it possesses properties so extraordinary, and that, to speak in common language, a mere nothing of it in point of quantity can be the source or the vehicle of enormous powers? Professor Tyndall says it must be a material substance, but perhaps not a form of ordinary matter. If it is composed like common matter, its particles or molecules do not touch; and in that case it will be difficult to avoid the belief that there is a still more subtle kind of matter filling up the interspaces. If it be matter not divided into atoms or molecules, but continuous, we may expect to find that it will exhibit many properties and peculiarities not yet discovered, differentiating it from matter in ordinary forms.

Faraday followed Newton in feeling an invincible objection to the notion that matter could act through empty spaces, and, as we find in his life by Dr. Bence Jones, he was fond of quoting the following passage from a letter of Newton to Bentley:—"That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, and without the mediation of anything else, by and through which this action and force may be conveyed from one to another is to me so great an absurdity, that I believe no man who has, in philosophical matters, a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

Faraday's own views on this subject were never very clear to other people. He recognised "lines of force," and spoke of "atoms" as centres of force, and not as so many little bodies surrounded by forces. The force was the atom extending indefinitely in all directions. According to these conceptions, "water is not two particles of oxygen (and hydrogen) side by side, but two spheres of power mutually penetrated, the centres even coinciding."* In the same place he said, "The force or forces constitute matter; there is no space between the particles distinct from the particles of matter."

We need not for present purposes pursue these speculations further. The wave forces mentioned communicate immense velocities to the molecules of matter, and these velocities are, in

* "Life of Faraday," vol. ii. p. 178.

fact, their powers. A gas—or common air, which is a mixture of gases—resists pressure and exerts pressure, because its particles are in vigorous, rapid, and ceaseless motion. Substances that are translucent, or transcalorescent, are so composed that ether waves go through them as water goes through a sieve. Bodies that do not allow light or heat to pass in this way, have their molecules set in motion by the impulse of the ether waves, and thus new forms of force are generated.

Tyndall's beautiful experiments on the powers of various substances to absorb heat and stop its radiation offer most instructive instances of the power exerted by small quantities of matter. Taking the absorption of one atmosphere of common air to be unity (1), it was found that this power was augmented thirty-fold when the same quantity of air was permeated by a little vapour of patchouli; lavender vapour raised it to 60 times, camomiles to 87, cassia to 109, and aniseed to 372. Upon these results Tyndall remarks "that the number of atoms in the tube (experimented with) must be regarded as almost infinite in comparison with those of the odours. . . . it would be idle to speculate on the quantities of matter implicated in these results. Probably they would have to be multiplied by millions to bring them up to the pressure of ordinary air. Thus—

> The sweet South
> That breathes upon a bank of violets,
> Stealing and giving odour,

owes its sweetness to an agent which, though almost infinitely attenuated, may be more potent as an intercepter of terrestrial radiation than the entire atmosphere from bank to sky." *

Wherever we find power exhibited, matter is in motion, and if the quantity of matter is infinitely small, and yet the power great, it is because the motion is infinitely quick. The waves of chemical force streaming from the sun are very short, and the quantity of matter acting in each oscillation, and tapping at the molecules on which it acts is inconceivably minute, but the taps are as inconceivably numerous and rapid. They are also rhythmical, and we know how the stone walls of a large building may be set in vibrating motion when an organ tone of the right pitch impels air waves to go on tap, tap, tapping till the whole fabric shakes.

We learn from these and similar facts that the wave forces can give great powers to infinitesimally small portions of matter, and that, as we are not able to place any limit that we can comprehend to the possible velocities of atoms and molecules,

* Tyndall, "Heat as a Mode of Motion."

so we are not able to assign any limit to the minuteness which would be incompatible with the exercise of effective force.

Becquerel has shown that when a membrane is moistened on each side by a different liquid, an electric wave force is set up, able to effect chemical decomposition. Thus the minutest part of the minutest gland, or of the smallest organism that is capable of assimilating external matter, is enabled to change the chemical condition, and pull asunder molecules or atoms that would resist the mechanical force of a steam-engine or a hydraulic press.

Unfortunately, we have no chance of seeing the ultimate atoms or molecules of matter. Chemists use the term molecule to denote the smallest quantity of any substance capable of existing alone ; but the definition is not quite satisfactory, because they have reason to believe there are many compound molecules that only exist in parts of more complicated combinations. Could we, by help of any apparatus, see ultimate molecules, the sight would be an astounding one ; for an extremely minute portion of any substance, however solid and quiet it might appear to ordinary vision, would be exhibited to us as composed of infinitely more particles than all the stars we can perceive in a clear sky, and all in motions as harmonious as those of the celestial bodies. When either compositions or decompositions are going on we should see hosts, by the myriad, rushing together, or springing apart, as the case might be. Eternal motion is the condition of life, whether of the smallest unit or of the entire universe. Nature, as Humboldt said, is ever arranging herself in new forms, and absolute stillness would be cessation of being.

The limits of visibility was one of the topics brought before the Royal Microscopical Society in February by the President, Mr. H. C. Sorby, in a remarkably able, and admirable, Annual Address.* Omitting the estimation of unavoidable errors in the construction of microscopical apparatus, and referring to researches by Pigott, Helmholtz, and Woodward, it seems that it is possible to distinguish the most favourable objects—alternate dark and bright lines such as in Nobert's test-plates—when they are as near each other as to be only $\frac{1}{112,000}$ of an inch apart, provided that several circumstances, which need not now be explained, are favourable. Minute spherules of about $\frac{1}{80,000}$ to $\frac{1}{100,000}$ of an inch may also be seen if their refractive power differs sufficiently from the fluid, or other medium in which they are immersed. It may, however, be affirmed that few objects less than $\frac{1}{80,000}$ of an inch in diameter can be seen;

* See "Monthly Microscopical Journal," for March 1876.

and of that size those only that are favourably placed. Mr. Sorby proceeded to inquire what sort of relation this power of microscopically assisted vision bears to the probable size of molecules of matter. He cited the results obtained by Stoney, Thompson, and Clerk-Maxwell, in attempts to calculate from different data the number of ultimate atoms in a given volume of any permanent and perfect gas at 0° C. and a pressure of one atmosphere. Thompson assigns as the greatest possible limit 98,320,000,000,000 in one-thousandth of an inch cube, which is $\frac{1}{1,000,000,000}$ of one cubic inch. Clerk-Maxwell, estimating the true number indicated by the phenomena of the interdiffusion of gases, made it 311,000,000; and Stoney, from his point of view, 1,901,000,000,000. The mean of these numbers is 50,260,000,000,000. In a letter received by the writer from Mr. Sorby, since the publication of his address in the "Monthly Microscopical Journal" for March, he assigns double weight to Clerk-Maxwell's calculations, for reasons that we need not stop to explain, and considers the number of atoms in a cubic $\frac{1}{1,000}$ of an inch of gas to be about 6,000,000,000,000, and that in the same space of liquid water the number of water atoms would be 3,700,000,000,000,000.

Water is essential to organic life: if an organism is thoroughly deprived of it, death ensues, though some creatures may be dried so as not to exhibit the least appearance of moisture, then pass into a dormant state, and become active again when more water and an appropriate temperature are supplied. The common rotifer and the *Anguillula tritici* have this property, and it is exhibited to some extent by that curious vertebrate, the Mud Fish, which survives an amount of drying that would be fatal to most animals as highly organised, though the baked mud in which it passes the hot dry season appears to prevent the desiccation from being carried too far for continuance of quiescent life.

If we say water is so valuable to organic creatures on account of its dissolving so many substances they need to be supplied with in a fluid state, we may be asked why water has such power, and it seems probable that they depend upon the immense number of its molecules, as well as upon their mode of aggregation. Each atom or molecule in motion tends to set adjacent atoms or molecules in similar motion; and a great number of small impulses, rhythmically repeated, easily set considerable masses of such bodies in fresh motions, differing more or less from those which belong to their own constitution. A child with a little hammer, tapping at a great log of wood, will in time set all the particles vibrating, and though each particle may move only through a small fraction of an inch, when the whole log vibrates, the total quantity of motion is enormous, because the small motion of each particle is multiplied by millions and millions—that is, by all the particles the log contains.

Among the complex substances which chemists are acquainted with, no one could be named more important to organic life than albumen, which we all know in the condition of white of egg; and its remarkable powers of utility in the growth and development of plants and animals depends upon its extremely complicated structure. It contains a multitude of atoms of carbon, hydrogen, nitrogen, oxygen, and sulphur. It is usually found slightly alkaline; and some chemists, like Gerhardt, consider white of egg as a definite compound of an albumen acid with sodic hydrate, and believe other sorts of albumen have an analagous composition. Omitting, however, the alkali, Mr. Sorby takes as a probable composition of albumen C_{72}, H_{112}, N_{18}, SO_{22}; the letters representing the substance above named, and the figures the number of atoms which they contribute to the structure. With this view of albumen he finds that in a cubic $\frac{1}{1,000}$th of inch of horn there are about 71,000,000,000,000 molecules of albumen. A molecule of this substance, though much larger than one of water, is far removed by its minuteness from any possibility of human vision; and as Mr. Sorby explains in his paper, light is too coarse a medium to enable them to be seen, even if we could add sufficiently to the powers of our microscopes.

When so many atoms of various substances are built up together to form a new substance, there is reason to believe that they are arranged in groups, each group having a definite constitution, and being a distinct entity, at the same time that it has an appointed place and a definite relation to the whole. Each group may be regarded as a system in which the atoms composing it are in ceaseless motion, exerting force upon their neighbours, and keeping within certain bounds, just as the planets do that circle round the sun. Each group acts as a whole upon other groups, and thus there are motions of groups as well as motions of atoms, subject to the same conditions of keeping within bounds.

Now, it is evident that the wave forces of which we have spoken have great opportunities of effecting changes in such complex structures. One form or mode of wave motion may strike with its myriad pulsations at a group of atoms, another may strike at certain atoms in the group, and by such means some atoms or groups may be thrown out of their courses, and then the rest may form a new pattern, or, if suitable atoms of another sort are at hand, may take them in to what may be called their social system, and modify it accordingly.

The phenomena of the nourishment and growth of plants and animals depend upon actions of this sort brought about by the wave motions of heat, light, electricity, and so forth. Reproduction is, as Claude Bernard explains, intimately connected with nutrition. A particle capable of germination or growth receives an impulse from a particle of an opposite sex, that is, of one in a different molecular condition, and development is stimulated and caused to take place so as to repeat with minor variations the parent forms. The well-known facts of inheritance show that, although the female germ and the stimulating male element—the ovule and pollen grain of a plant, for example—are very minute, they are big enough to contain, in some form, or way, forces which cause all fresh matter that is assimilated to arrange itself so as to reproduce a series of parts repeating for generations with marvellous fidelity the parental types.

The same thing is noticed with animals in which the same species or the same race is reproduced from one generation to another with remarkable accuracy, extending to minute and often unexpected detail. For information on this subject the reader must be referred to the works of Darwin and other writers. What we have now to consider is whether the germ particles and sperm particles can possibly be conceived to contain enough molecules built up in definite patterns, so that, as Darwin in his theory of Pangenesis supposes, they can supply *parents* enough to enable us to regard each portion of a complex organism, plant or animal, as composed of their lineal descendants. "If," says Darwin, "one of the simplest Protozoa be formed, as appears under the microscope, of a small mass of homogeneous gelatinous matter, a minute atom thrown off from any part, and nourished under favourable circumstances, would naturally reproduce the whole; but if the upper and lower surfaces were to differ in texture from the central portion, then all three parts would have to throw off atoms, or gemmules, which, when aggregated by mutual affinity, would form either buds or the sexual elements. Precisely the same view may be extended to one of the higher animals, although in this case many thousands of gemmules must be thrown off from the various parts of the body." *

To compose a plant under this theory, the seed must contain gemmules which attract suitable matter to form root-fibres; other gemmules that in like way cause cells to grow and aggregate to make a fibrous stem, others to supply the sap, others to cause the growth and development of the leaves, flowers, and finally to supply the ovule and the pollen with a complete set of gemmules to carry on the process from one generation to

* "Animals and Plants under Domestication," vol. ii. chap. xxvii.

another; and as certain peculiarities of distant ancestors sometimes suddenly appear in their descendants, the ancestral gemmules must be sufficient in number to last for many generations, or they must act as parent cells and produce other cells.

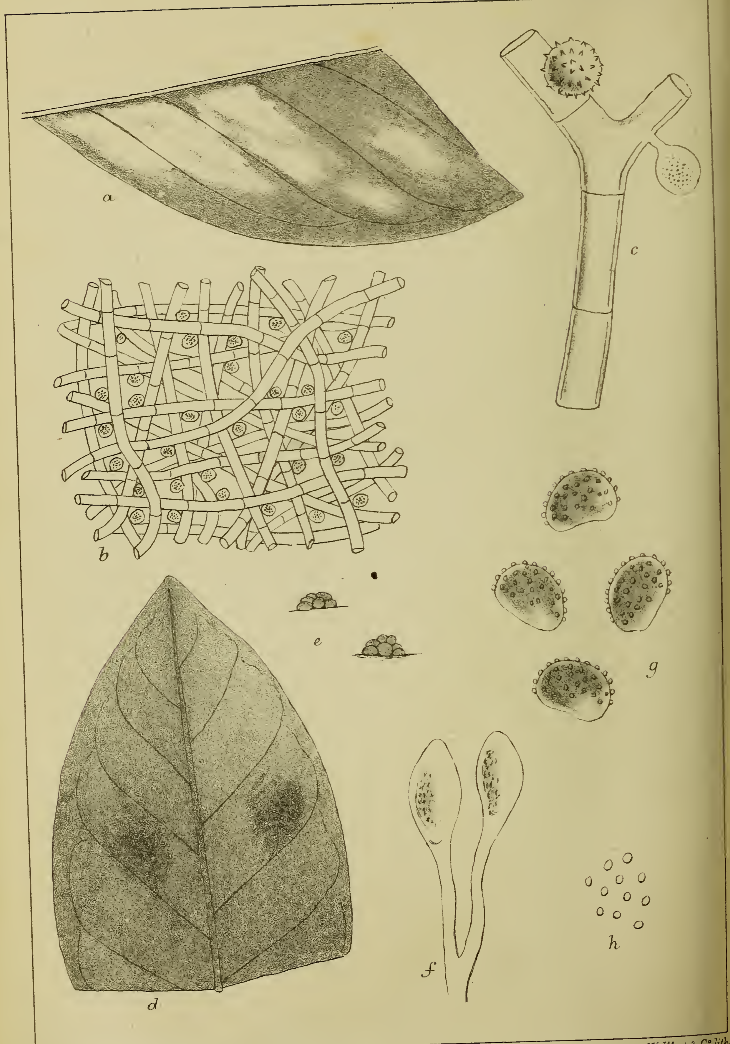
Mr. Sorby applied himself to this problem, and sought to find what quantity of molecules existed in the quantities of matter that acts as germs and sperms. Supposing each gemmule contained a million molecules of the albuminous compound that is the physical basis of life, Mr. Sorby finds that "one thousand such gemmules massed together would form a sphere just distinctly visible with our highest and best magnifying powers." "If," he adds, "the gemmules were of much greater or much less magnitude, it appears to me very probable that Darwin's theory would break down from two opposite causes, or would need very considerable modification, because, if much greater, their number would be too few to transmit sufficiently varied characters, and if much less, they would scarcely contain enough of the ultimate atoms of matter to have a sufficiently varied individual character to transmit, since, of the assumed million ultimate molecules, only eighteen thousand would be of a true protoplasmic nature, the rest being water in molecular combination." Taking the $\frac{1}{6,000}$ of an inch as the mean diameter of a single mammalian spermatozoon, Mr. Sorby calculates it might contain $2\frac{1}{2}$ millions of such gemmules, and, "if one of them were lost, destroyed, or fully developed at the rate of one in each second, this number would be exhausted in about one month; but since a number of spermatozoa appears to be necessary to produce perfect fertilization, it is quite easy to understand that the number of gemmules introduced into the ovum may be so great that the influence of the male parent may be very marked, even after having been, as regards particular characters, apparently dormant for many years."

Again, taking the germinal vesicle of the mammalian ovum as $\frac{1}{1,000}$ of an inch in diameter, "it might contain 500 millions of gemmules," and "if these were lost or fully developed at the rate of one in each second, this number would not be exhausted until after a period of seventeen years." If the whole ovum, about $\frac{1}{150}$ in diameter, were all gemmules, the number would be sufficient to last, at this rate of one per second, for 5,600 years! This is, however, not probable; but Mr. Sorby's remarks have completely removed all doubts as to its physical possibility from the Darwinian theory, and they prompt us to a wonderful conception of the powers residing in minute quantities of matter.

The student of nature stands surrounded on all sides by infinities. He can imagine no bounds to space or time; see no traces of a beginning, discover no symptoms of an end. There is an eternal flow and motion throughout the universe, a ceaseless change from power in the potential to power in the active form, and back again from the active to the potential—nothing added, nothing stationary, and nothing lost. Such is the aspect of the physical world, but what of the world of thought and will? Here we pause before a door of difficulty, and have no key to open. Let Du Bois-Reymond point it out:—

"Suppose we had arrived at an astronomical knowledge of the human brain, or even of an analogous organ in an inferior creature whose intellectual activity was limited to the sensations of well-being and discomfort. So far as regards the material phenomena of the brain our comprehension would be perfect, and our intellectual need to seek for causes would be satisfied in the same degree as it would be in regard to contraction and secretion, if we possessed astronomical knowledge of a muscle and a gland. The involuntary acts which emanate from nervous centres, without being necessarily connected with sensations, such as reflex and associated movements, respiration, tonicity, and lastly, the nutrition of the brain and spinal marrow, would be entirely known to us. It would be the same with the material changes that always coincide with intellectual phenomena, and which probably are conditions indispensable to them. And surely it would be a great triumph of science if we could affirm that such intellectual phenomenon was accompanied with certain movements of atoms in certain ganglionic cells and certain nerve tubes. What could be more interesting than to direct our intellectual vision inwards, and see the cerebral mechanism in motion corresponding with an operation of arithmetic, as we can watch that of a calculating machine; or to perceive what rhythmical movement of the atoms of carbon, hydrogen, nitrogen, oxygen, phosphorus, &c., corresponds with the pleasure we feel from musical harmony; what eddying currents of the like atoms attend the acme of delight, and what molecular tempests accompany the horrible suffering that ensues from irritation of the trigerminal nerve . . . ; but as regards the mental phenomena themselves, it is easy to see that, after acquiring an astronomical knowledge of the brain, they would remain just as incomprehensible as they are now. In spite of such knowledge, we should be arrested by those phenomena as things that are incommensurable. The most intimate knowledge of the brain to which we can aspire would only reveal to us matter in movement; but no arrangement, and no movement of material particles, can form a bridge to conduct us into the domain of intelligence. Motion can produce nothing but motion, or enter into the condition of potential energy. Potential energy in its turn can produce nothing besides motion, the maintenance of an equilibrium, the exertion of pressure on traction. The total quantity of energy remains always the same. In the material world nothing can go beyond this law, and nothing can do less than it requires. The mechanical effect is precisely equal to the mechanical cause that exhausts itself in producing it. Thus the intellectual phenomena which flow from the brain beside of, but in addition to, the material changes that occur in it, are, to our intelligence, wanting in a sufficient reason. These phenomena remain outside the physical law of causality, and that is sufficient to render them incomprehensible." *

* Du Bois-Reymond, "Revue Scientifique," Oct. 10, 1874, p. 342.



M.C. C. del.

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Coffee Leaf Diseases.

TWO COFFEE DISEASES.

By M. C. COOKE, M.A.

[PLATE CXXXV.]

THE cultivation of Coffee under no circumstances is such a profitable investment as to offer great inducements for speculation; but, unfortunately, during the past six or seven years, no less than three resolute and determined enemies to the coffee crop have successively made their appearance, and spread dismay amongst the coffee planters. One of these, the Coffee Borer, though by no means of the least importance, it is not our present intention to molest. The other two, being obscure members of the vegetable kingdom, alone come within the province of the botanist to describe.

Until within a few weeks it was not known in Europe, at least as far as we can learn, that more than one fungoid pest was making havoc amongst the coffee plantations in the East. Recently communications from planters in Mysore were sent to the Government of India, and these reports, together with specimens of infected leaves, have been transmitted to the India Office in London, undergone investigation, and resulted in a Report, which establishes, without doubt, the existence of a second fungal disease, which, in India at least, seems to be more destructive than the one previously known.

The "coffee-leaf disease," as it is usually termed, in distinction from the "coffee rot," is caused by a parasitic fungus. Of this there is not the slightest reason to doubt, although some have held to the opinion that it is the result, and not the cause, of the disease. There being still a few who hold similar views respecting the potato disease, it is by no means surprising that the same theory should be applied to the coffee plant. Its first appearance in the island of Ceylon was in the year 1869; and recent Reports from the planters in Mysore date its first appearance in Continental India in the same or following year. When suffering from this disease the leaves are marked by brownish somewhat rounded spots (Pl. CXXXV. fig. *d*), the under surface of which is covered with minute orange dust, consisting of the spores of the fungus. These spores only appear on the under surface of the leaf, but the spots are observable also on the upper surface. The fungus is truly an endophyte living within the tissues of the plant, and expanding outwards. To its presence Dr. Thwaites attributes stains which are produced on the bark of the young branches, and the pale translucent spots which are to be seen on the leaves previous to the outbreak of the orange-coloured spores.

From the delicate mycelioid filaments which constitute the root-like portion of this parasitic fungus, threads arise in bundles which burst through the cuticle and produce at the tips of the threads the orange-coloured spores. Each of these spores has a somewhat irregular form, ovate, pear-shaped, or more or less of a kidney shape, covered externally when mature with minute globose warts, principally on the convex side, the shorter side being usually smooth (Pl. CXXXV. fig. *g*). When fresh they are of a bright yellow or orange colour, but this speedily disappears in drying. The longest diameter of the spore is from ·035 to ·04 of a millemètre, and that of the warts about ·003–·004 of a millemètre. We have observed these warts to leave the surface of the spores, and float in the medium in which the spores were examined as minute free globose bodies (Pl. CXXXV. fig. *h*). This was not noticed by Messrs. Berkeley and Broome in their examination of spores from Ceylon. Probably it may be dependent on the degree of maturity at which they had arrived. In the early stage the spores are smooth, and may be seen attached to the apices of the simple or branched threads on which they are produced (Pl. CXXXV. fig. *f*). When mature, the attachment is so slight that the least touch is sufficient to disengage them, and they appear to lie in little clusters on the discoloured spots, ready to be dispersed by the slightest movement of the leaves.

The fungus thus described was first determined and named by Messrs. Berkeley and Broome in the "Gardener's Chronicle,"* shortly after its first appearance in Ceylon, under the name of *Hemileia vastatrix*, and was regarded as a very interesting addition to science, from its forming a link between moulds and rusts.

The mode of germination has not been observed in this country. It is scarcely probable that vitality in the spores will extend beyond the loss of the yellow colouring of the endochrome. In Ceylon, however, Dr. Thwaites reports that it is not difficult to induce germination. Mature spores removed from a diseased leaf and laid upon charcoal kept continually moist, he says,

* "Gardener's Chronicle" for November 6th, 1869.

commence to germinate in a few days. "This process consists in the spore becoming somewhat enlarged, and its contents converted into one or more globose translucent masses. From each of these a filament is developed, which grows very rapidly, and becomes more or less branched. At the termination of some of these branches secondary spores are produced in the form of radiating necklace-shaped strings of little spherical bodies of uniform size, and this form closely resembles the fructification of an *Aspergillus*." Another observer in Ceylon (Mr. Abbay) has seen another form of secondary spores arranged in single rows of spherical bodies, a good deal larger than those radiately arranged, but still exceedingly minute. These inconceivably numerous secondary spores may be easily carried by the wind into surrounding districts, and thus convey infection to distant plantations. In what precise manner infection is accomplished is still unknown. The minute secondary spores may be absorbed by the rootlets, or they may enter by the stomata, or they may even germinate on the surface and insinuate their growing points through the natural orifices of the leaves. Neither is it yet known whether the successive attacks to which a plant is subject are all the result of one inoculation, or whether a fresh infection precedes each outburst of the disease.

In some features there is a similarity between this fungus and the red rust of cereals. Both of them burst through the cuticle and appear on the surface as an orange-coloured dust; in both the spores are at first produced on pedicels: but beyond this there is great divergence, especially in the mode of production of the secondary spores, of which there is no similar instance amongst the *Uredines*. The germination of a large number of species amongst the *Uredines* has been observed, but hitherto in no instance have minute secondary spores, growing in chains, been recognised. There is doubtless another feature in which the coffee rust resembles the wheat rust: it is not likely to yield to the application of sulphur, as the white moulds usually do. Sometime since the application of a diluted Condy's fluid was recommended as an efficient check for the Hollyhock disease, and it is not improbable that what would succeed with the *Puccinia* would be beneficial in the case of the *Hemileia*; but hitherto it has been affirmed that nothing has proved effectual in checking the ravages of the coffee disease in Ceylon. Many of the coffee planters in Mysore declare that this disease in their plantations has not sensibly affected the quality or quantity of the crops; in others the contrary is affirmed.

The second, and more recently known, coffee disease has not at present been recognised in Ceylon, although Indian planters declare that it certainly has been present on coffee estates in

Mysore almost as long as the "coffee-leaf disease." Mr. G. Porter says that "it is very prevalent in the Mulnad portions (and more especially in the Kadur district) of the Mysore country. It is known as the 'kole roga,' or 'black rot,' and not only does it attack coffee, but it does great havoc in the betel-nut gardens. He was told by the natives that they had hardly ever known a wet season without it, some years worse than others. It is this disease that coffee suffers far more from than from that of the *Hemileia vastatrix*. It makes its appearance about July, when the leaves of the trees affected by it get covered with a slimy gelatinous matter, and turning black drop off; the berries likewise rot and fall in clusters. He estimates that the planters lose nearly one quarter of their crop each year by this plague. Gangs were sent round last monsoon to collect the diseased leaves, which were carried off the estate and burnt; and although this did some good, it was not the means of abating it to any extent."

The leaves affected with this "rot" are spotted on the under surface with large greyish-white irregular patches, sometimes occupying nearly the whole surface, sometimes in spots limited by the larger veins (Pl. CXXXV. fig. *a*). These patches are quite smooth to the naked eye, with all the appearance of a superficial incrustation. When moistened, the entire spot can be removed with the point of a lancet, by stripping it in a delicate hyaline film, somewhat like a film of gold-beater's skin, showing no attachment to the leaf, unless of a very slender and superficial nature; indeed, the film may be rolled up under the thumb and finger. Under the microscope this film is found to consist of a closely interwoven web of hyaline septate filaments, often branched, and crossing in all directions (Pl. CXXXV. fig. *b*). They are usually from ·005 to ·0075 of a millemètre in diameter. On the upper surface these threads are studded at irregular intervals with small globose echinulate spores, which are seated upon the threads, without any visible pedicel, although when first formed there appears to be a short stem, which is ultimately absorbed (Pl. CXXXV. fig. *c*). These spores are about equal in diameter to the diameter of the threads. The threads and spores seem to be agglutinated together into a film by some gelatinous medium, so that not a spore or thread can be removed from the mass without difficulty. In this feature the "rot" differs from nearly all the *Mucedines*, in which the spores are so slightly attached that they float away on the application of moisture, whilst in the present instance no application of fluid avails to disturb a single spore.

In order to examine the fungus in as complete a manner as possible, a portion of the leaf is immersed for twelve hours in water, but this does not dissolve the mucus so as to free the spores. Whether examined in water, spirit, or glycerine, the results are the same ; but in nitric acid the threads are at first more distinct, but gradually become absorbed into an indistinct mass. When the film is stained with aniline or roseine, the threads and spores are brightly coloured by the medium, so that the details may be better observed. There is, however, still considerable difficulty in penetrating the film with a high power, and the threads will not separate.

From an examination of this fungus, with a view to the determination of its scientific relationship, we have come to the conclusion that it has no very close affinities, that it is not only specifically new, but will have to be accepted as the type of a new genus.* Whether it is in itself an autonomous species, or a condition of some other and higher form, cannot be determined from present information ; at any rate, it is so far complete as to possess a vegetative and reproductive system. The globose echinulate bodies have all the characteristics of spores, but more than this cannot be affirmed until some one is fortunate enough to observe their germination, or all endeavours to do so should fail.

The principal scientific question which presents itself in relation to this fungus is its relationship and affinity. Two or three suggestions have already been offered on the subject ; although made without any microscopical examination of the plant itself, they are worthy of a passing notice. One suggestion is that the supposed fungus may be an imperfect condition of some lichen. It may be true that low forms, or imperfect states, of lichens are sometimes found on the living leaves of growing plants, yet the structure is hardly such as those lichenoid bodies assume. Considerable emphasis is sometimes placed on the presence of gonidia in the lichen thallus as distinguishing it from fungi. There is no manifestation of such bodies in the present instance, and it would be more satisfactory for such an objection if a similar authentic instance could be adduced of a destructive leaf-parasite which is an undoubted lichen. Another suggestion has been offered that it may be a low form of Hymenomycetous fungi. If so, it should at least give some indication of its relationship. As spores are undoubtedly present, there should also be basidia, bearing these spores in pairs or quaternate ; at least, there should be some evidence of an approach to such low hymenomycetal forms as *Exobasidium* or *Hymenula*. Probably it was some such organism as *Exobasidium* which was thought of when this suggestion was made, but, certainly, we can observe no relationship whatever between them.

* "*Pellicularia Koleroga*"—Cooke in Grevillea, iv. p. 116. "On the Affinities of *Pellicularia*," in Grevillea, iv. p. 134. "Report on Diseased Leaves of Coffee and other Plants."

Apropos of the suggestion which has been offered through the medium of a horticultural newspaper, that the "black rot" appears to be the *mycelium* of some fungus, it will be sufficient to remark that the term "mycelium" is, by general consent, confined to productions which consist of barren threads. The presence of spores, in this instance, clearly removes the production beyond the limits of the term "mycelium." Unless terms are employed with their recognized meaning and limitations, some explanation should accompany their use to prevent misconception.

The conclusion at which we have arrived appears to us the most tenable one, that the fungus in question belongs to the *Hyphomycetes*, or moulds. In habit and external appearance it strongly reminds one of the white mould which precedes many species of *Erysiphe*, such as the one so common on peas in the autumn, or that which precedes *Uncinula* on the leaves of the maple. Even under the microscope there seems to be some kind of relationship; the interwoven, septate, colourless branched threads are present, but there is an addition of a somewhat gelatinous medium, which binds the threads together into a pellicle. The spores and their mode of production are different, and this, in the *Hyphomycetes*, is a most important distinction. In *Oïdium* the spores are produced in chains, in the present species singly. It is very true that the structure, as seen in a drawing, resembles closely that of some species of *Zygodesmus*, but there is a peculiarity in the threads of many of the species in that genus that the threads are cut, as it were, nearly through at short distances, or abruptly bent, of which there is not the slightest indication here. The spores are very similar in size and form, but there are two or three features which appear to us conclusive for rejecting the coffee rot from this genus. In all the species of *Zygodesmus* the threads are free from any investing medium, the spores are pulverulent, and, moreover, the threads are more or less coloured. Further than this, all the species occur on dead wood or leaves, and in no instance is a species parasitic on living leaves. Although too much reliance is not to be placed on this fact, it is nevertheless noteworthy that in genera in which the species are parasitic on living plants there is seldom an exception to this rule, and so in genera which contain species found on dead substances parasitic species are not found. In illustration of the former we may cite *Peronospora*, *Ramularia*, and *Erysiphe*, and of the latter *Dactylium*, *Sporotrichum*, and *Zygodesmus*.

The presence of the gelatinous element which binds together the threads and spores into a thin pellicle, which is easily separable from the matrix when moist, is an important feature in determining the affinities of the "coffee rot." In the genus

Amphiblistrum of Corda there is said to be such a gelatinous medium. In many species of *Fusisporium* there is something of the same kind; in *Alytosporium* as constituted by Link, and in some other genera allied to *Sporotrichum*. Still, from all these there are such manifest points of divergence that no one would venture to associate the present species with any of them. Hence no other course appeared to be open to us but to constitute *Pellicularia Koleroga* the type of a new genus, allied to those just alluded to, but distinguished therefrom by its parasitic habit, sessile, echinulate, globose spores, and the freedom with which it separates from the matrix. Whether or not mycologists will accept this as a sufficient distinction, the present course has not been adopted without much consideration.

The fact of an epiphytal fungus, which does not penetrate the tissues of the leaf, being so destructive to the foster plant, may at first seem strange, until it is remembered that in plants with coriaceous leaves all, or nearly all, the stomata are confined to the under surface of the leaf. If, therefore, a filmy substance like the present fungus overspreads the under surface of the leaf, and securely seals up all the stomata, it is but reasonable to expect, not only that the leaves should fall, but that the plants should suffer injury. In such diseases as that which affects the hop, and which is but too well known to hop-growers in this country, the chief destructive action lies in the closing up of the orifices of the leaf by the woolly coating of mycelium produced by the fungus.

From the similarity of habit and growth in the coffee rot to that of the hop mould, and, we may also add, of the vine mildew, it is extremely probable that the remedies which have been applied in the latter instances with success would be more or less advantageous in the former. It is now generally admitted that the application of the flowers of sulphur, by dusting over the leaves, is the most effectual remedy yet discovered for hop mould and vine mildew. It should certainly have a fair trial also in the case of the "coffee rot," and the presumption is strongly in its favour.

We have stated sufficient to show that the coffee plant in Mysore and in Ceylon is exposed to considerable danger from the two persistent enemies above described. Some plants of the Liberian variety were sent out some time since in the hope that its apparently stronger constitution would prove impregnable to the *Hemileia*. At first they progressed favourably, but subsequent information dispelled the hope, for the coffee disease had attacked them also. Unless some remedy is discovered there is reason to fear that even if the cultivation of coffee is continued in these places it will be unremunerative, or, the costs and risks of the venture being so much increased, a corresponding increase in price will be a natural result. We have the experience of the vine and potato diseases, and more recently of the hollyhock disease, to warn us how surely and speedily the area of infection is extended, and though for the present some coffee-producing countries are free, one or other of the pests may at any time make its appearance where it has never been known before.

DESCRIPTION OF PLATE CXXXV.

a. Portion of leaf affected with "black rot."

b. Fragment of pellicle formed by the fungus *Pellicularia koleroga* × 500 diam., showing threads and spores.

c. Small portion of thread further magnified, with young and mature spore.

d. Portion of leaf affected with *Hemileia vastatrix.*

e. Tufts of spores as they appear clustered on the spots, slightly magnified.

f. Young threads and spores × 500 diam.

g. Mature spores of *Hemileia vastatrix* × 500 diam.

h. Globose bodies, or warts, from the surface of mature spores of *Hemileia* × 500 diam.

AMONG GLACIERS RECENT AND EXTINCT.

By the Rev. W. S. SYMONDS, F.G.S.

IF we would see *King Frost* on his throne, we must travel to the Arctic or Antarctic regions. Of the latter little is known, save that there are mountains rising to the height of 15,000 feet, that the Antarctic continent is at present ice-bound and going through a glacial epoch, and that ships are stopped by pack-ice before reaching the 70th degree of latitude. Victoria Land, which extends from 71° to 79° south latitude, was ascertained by the exploring expedition of Sir James Ross (1841) to be fringed by an enormous barrier of ice; while the inland country rises from 4,000 to 15,000 feet above the sea, as in Mount Melbourne, and the crater of Mount Erebus is elevated to the height of 12,000 feet. Graham's and Enderby's Lands, in the Antarctic regions (lat. 64° and 68° S.), are situated in the same parallels of latitude as are those regions of the northern hemisphere which are inhabited by man and herds of wild animals; but the Antarctic lands are not known to possess a single land animal, and are wild, wintry, and desolate in the extreme. Frost reigns everywhere! In the Arctic regions of the distant North voyagers have explored much the seas and border lands—the home of Arctic men, and the abode of the musk ox, the polar bear, the walrus, and the reindeer.

The accounts of Greenland are remarkable. Some travellers who have penetrated a few miles into the interior of southern Greenland describe it as occupied by one vast glacier, and state that in 70° N. the land of the interior is covered by one vast ice-sheet of unknown depth, which conceals and obliterates all indications of hill and valley. This vast mass of inland ice is in constant motion, creeping and advancing slowly, but with different velocity, in different places towards the sea. Near the sea it presents "ice-walls" rising sometimes to the height of 3,000 feet, and from these break off the iceberg and icefloe, with a crashing and then a roar like the discharge of a park of artillery. Some of the bergs ground in the fiords and break up slowly, others sail off to the ocean, sometimes rising to the height of 200 feet, looking like vast glassy towers and minarets as they move steadily out to sea. We learn also from the recent expedition of the German Expedition of the *Germania* and *Hansa*, that the north-eastern district of Greenland is a great contrast, as regards climate, glaciers, and animal life, when compared with southern Greenland. In 75° 29′ of latitude N. they found abundance of reindeer. In 73° 13′ N. a large fiord was discovered and entered by the *Germania*, and she ascended it for seventy-two miles. The further they ascended the warmer became the temperature of the air and the sea. The scenery of the country of South Greenland, about 70° N., is described by Mr. Whymper as "completely covered with glacial ice up to a great altitude, and at the summit formed a dead level." The scenery as described from the reports of Captain Koldeway and Dr. Laube, up this north-eastern fiord entered by the *Germania*, is "Alpine," "beautiful," and "imposing." "An unknown land—the real interior of Greenland—revealed itself before their astonished gaze." Some mountains were ascertained to be as high as the Matterhorn (14,000 ft.), and one 7,000 feet high was ascended. Numerous glaciers, waterfalls, and cascades came down from the mountains, and reindeer and musk oxen roamed about in herds. Ermines and lemmings were also met with, but the Esquimaux appear to exist no longer in this district.

The experiences of the crew of the *Hansa* were very different. She was driven far to the south by winds and currents. *Frost* caught her by the middle of Sept., and the ship was frozen hard and fast, without hope of escape from a winter in the ice. In Oct. the ice crushed up the ship, and the *Hansa* sank, leaving the crew on an icefloe. On this icefloe for 200 days they lived and drifted, struggling continually amidst ice and storms, and suffering frequently from bitter hunger. In May they were enabled to launch their boats, and on June 13 reached the station of Friedrichstal, after enduring the most terrible hardships.* We learn from such voyages the importance the glacialist must attach to the drifting of icefloes and icebergs. The superficial area of the floe on which the crew of the *Hansa* were saved was upwards of three square miles; and arctic voyagers, in the high northern latitude of Spitzbergen, have seen bergs drifting along loaded with thousands of tons of rocks and earth, which on melting fall to the bottom of the sea, or against some coast or island near which the berg is stranded. Such must have been the history of the transportation of the boulders which have come from Norway and Sweden, and which were deposited on the coast of Norfolk, above the submerged Cromer

* See "Proceedings of the Royal Geographical Society," vol. xv. No. 2, July 1871, p. 102, &c.

forest bed and the skeletons of its extinct mammalia. Seeds and berries of herbs, and shrubs too, must be often carried on ice-rafts out to sea; and we may owe our Arctic plants which still linger on our mountain summits in Great Britain to such migrations in glacial times. Bears and wolves have often been seen and heard on icefloes, hundreds of miles from the shore; and the crew of the *Hansa* are not the first men who voyaged upon a floe.

I shall allude to Greenland again as revealing another history. It is the lot of few men to behold such ice and frost phenomena as I have alluded to. Ordinary mortals must be content with ascending mountain heights in more temperate latitudes, where the atmosphere becomes colder as we ascend; for even in tropical regions there are mountain heights where frost does its work. In the Swiss Alps the snow line is about 8,500 feet above the sea, so that in the lofty recesses of the high Alps, which reach from 12,000 to 15,000 feet in height, the moment the sun goes down, frost sets in, even in the summer time, and the icicle droops from the rock, at night, where, in the daytime, dripped the runlet. In Switzerland, as is well known, we may ascend to regions on the summer snow-line, where comfortable hostels are established, and we may investigate snow and ice and glaciers. Such is the Inn of the Riffel, above Zermatt and the great Gorner Glacier, and from near which we visit the Monte Rosa glaciers, and behold a panorama of snow mountains forty miles in diameter. Such snow mountains are the great feeders of the glaciers of the Alps. Everyone knows what a glacier is, and how they are rivers of ice, frozen masses, which descend sometimes for a distance of twenty miles from Alpine heights to the valleys below. We need not enter into the disputed cause of their *motion*. I propose rather to direct attention to phenomena presented by these products of frost which come within the observation of all travellers in Alpine regions, and the observation of which greatly enhances the enjoyment of Alpine travel. Unlike the continental ice of South Greenland, above which no rock rises above the ice to become shattered by the frost, or weathered by the gale, to send down its débris upon its glassy bosom, in Alpine districts the mountains rise sometimes thousands of feet above these rivers of ice, as they creep down the valleys and gorges; and great masses of rocky débris fall on them, the result of the continuous and alternate action of thaw and frost. These accumulations are termed *moraines*: and there are lateral moraines, which are piled on the sides of a glacier; medial moraines, which are formed when two streams of ice meet; and the terminal moraine, where the débris is deposited at the termination of the ice.

Now with regard to *moraines*, I advise every traveller among glaciers who would add to the enjoyment of his tour to learn as much as possible, by the aid of geological maps, the evidence of a trusty guide, and above all by personal observation, what the rocks consist of which overhang a glacier at different localities along its course. For example, the great Aletsch glacier is more than twenty miles in length, and in its moraines are rocks of totally different formations: some are wanderers from the source of the glacier among the great snow-fields; others are derived from rocks *in situ* nearer to the foot of the glacier at the Bel Alp. Again, at the Zmutt Glacier near Zermatt, a rock fragment which has fallen from the Matterhorn is a volcanic rock, and differs in toto from a fragment from the Dent Blanche. In many instances, and many localities, these ice-borne masses render evidence that the ice of existing glaciers once rose far above their present surface, and, stranded high upon mountain flanks, sometimes a thousand feet above the glacier, or the site of a glacier long since melted away, it is often of importance to the geologist to know the home of the parent rock from which this or that wanderer has been derived. This is especially the case in hilly countries, where much débris has fallen from the rocks into the mountain vales. More than once it has occurred to me to be sent to see old glacier moraines which were no moraines at all, and which would not have happened had my friend known the difference between atmospheric débris and the erratic masses which more or less are generally to be traced in true moraines. It is well also to accustom the eye to recognise another witness of the existence of a glacier—viz. the "*roche moutonée*," so called from the supposed similarity to the round and smooth outline of a sheep's back when newly sheared and when lying down. These *roches moutonées* are portions of the rock, *in situ*, over which a glacier has passed, and owe their round and smooth outline to the action of the ice continually grinding over them and planing off the inequalities. *Blocs perchés* also are rock masses which have been carried upon a glacier often for long distances, and left perched, on the melting and retiring of the ice, far from the parent rocks from which they were derived. Among the Mont Blanc Alps are rocks of different character. There are granite rocks, gneiss rocks, conglomerates and various others; and it is very striking to see a *bloc perché* of Mont Blanc granite, glistening white in the sun, perched upon some dark rock on the side of a hill, far from the noble mountains from which it has travelled.

Some years have passed away now since I accompanied my friend Sir William Guise, who also loves the lore of the rocks, to examine glacial phenomena among some of the noblest of Alpine scenery, to collect Alpine minerals and fossils, Alpine plants, Alpine butterflies, and Alpine everything. We first visited the glaciers of the Rhone and Viesch, and the great Aletsch glacier, and later on we went to the Monte Rosa district and its glaciers. At the foot of the Rhone glacier we saw proofs of the recent gradual diminution of the ice in that district, when we examined the terminal moraines which are arranged concentrically one within another, showing the retiring, step by step, of the ice foot. We soon found that the glaciers of the Alps are not everywhere shrinking at the present time. The Gorner and Findelen glaciers, which descend from the Monte Rosa snow-fields, are *increasing*, for we found the great Gorner glacier towards the base ploughing up the green turf and cherry-trees thereon. Men too are still living who can remember encroachments on considerable tracts of land, and even that the Gorner glacier thrust boulders through the walls of chalets, some fifty or sixty of which were destroyed by the protrusion of the ice. The Zmutt glacier, too, which takes its name from the vast masses of rock which cover its lower extremity, should be visited to see what an amount of moraine matter is travelling slowly onwards towards the rushing river which flows in a torrent from its icy interior. Above it rises on one side the magnificent Matterhorn, which sends down masses of greenstone to the glacier, to travel onwards with the gneiss of the Dent Blanche and Col d'Erin. Our guide over this glacier was one of those who the year before went to assist in bringing in the mangled remains of Mr. Hudson, Mr. Hadow, and Michael Croz, after that fearful fall from a precipice of 4,000 feet, on the side of the Matterhorn above the glacier. The body of Lord Francis Douglas they never found, and it was supposed to have fallen into a crevasse. I need hardly allude to the grand scenery around Zermatt and the Riffel, with Monte Rosa and her glaciers, the Lyskamn, the Breithorn, the Matterhorn, the Dent Blanche, and the Weishorn as seen from the Gorner Grat and the Cima di Jazi, which itself is a scene of enchantment as you look upon these magnificent mountains on whose summits snow and frost reign supreme, and which are seldom trodden save by the chamois, or floated over save by the lammergeyer. Here, too, you stand face to face with the tremendous eastern precipice of Monte Rosa, while 6,000 feet below lies the Macugnaga glacier, and away, far below that, the green vineyards of sunny Italy. Yet even here, though surrounded with snow mountains, from which flow great rivers of ice, around the Riffel, on the flanks of Monte Rosa, or at the Gorner Grat, every place that is free from snow reveals evidence of the once greater extension of far larger glaciers in roches moutonées, blocs perchés, and ice action of other days. But before alluding to the phenomena of *extinct* glacial action more fully, allow me to say that I know of no glaciers where the results of ice action, or glaciation, can be studied in greater perfection than among the Mont Blanc Alps around Chamounix. I visited this district during the autumn of 1875. Here the changes are most marked within the last thirty years. Sir Wm. Guise pointed out to me the spot where the foot of the glacier of the Mer de Glace then extended beyond the Arveiron's source. It has retreated nearly a hundred yards. Higher up, near the Montanvert, the glacier has sunk twenty feet below the rock from which he, thirty years ago, stepped upon the ice. Such has been the gradual diminution within that period. The older guides confirm Sir William's statement, and the correctness of his observations. Here all around we may study moraines far from the glacier, roches moutonées of gneiss, worn and polished as if the ice had vanished suddenly the day before, and bare and brown as if scathed by fire; with here and there a bloc perché of white granite which has travelled from Mont Blanc.

Near the source of the Arveiron the Mer de Glace has piled up a great terminal moraine, rising into a long low hill, one side of which is covered with trees. This hill is made up of masses of rock of all sizes, which once were wanderers upon the slow moving glacier when it thrust its ice foot beyond the present termination. One great erratic of Mont Blanc granite has been marked with a large red 2, by the Geological Society of Switzerland, to prevent its being blasted by gunpowder for building purposes. This erratic is as large as a good-sized cottage, and is an example of the size of the rock masses which now and then fall from the hills above the glacier, and are carried forward often for many miles upon the ice. Numerous bloc perchés are stranded on the flanks of the hills on the right bank of the Mer de Glace, as you descend from the Jardin to the Chapeau, which the observer may at once see do not belong to the rocks on which they lie stranded. There are some rock masses, too, which have been derived from different localities, now perched on the ice towards the foot of the glacier where it comes down into the valley among chalets, and pine woods, and Alpine gardens.

Such are some of the modern phenomena the geologist learns to observe among glaciers and ice action. These, however, are merely subsidiary to the interest with which we pursue the examination of the evidence that there were, in former days, glaciers of *colossal dimensions*, which filled the great vales of Switzerland with ice, and which reached from the Alps to the Jura, across where is now the Lake of Geneva. To this strange history belongs the transportation of the celebrated Mont Blanc granite boulders, which are stranded 900 feet above the Lake of Neufchâtel, and of which the Pierra à Bot is the most remarkable. Again, on the east of the Lake of Geneva, opposite Bex, are the great blocs perchés of Monthey, which lie stranded against the hills that bound the Rhone valley. Most of them are of granite, and, if not quarried for building purposes, there was one which was calculated to contain some 20,000 tons of granite. They lie perched several hundred feet above the Rhone, and are the relics of a great glacier that once filled the valley, during the period known to geologists as the *Glacial Epoch.* Nor is the evidence of the former great extension of glaciers deficient near Chamounix for those who look out for such proofs. As already observed, high above the Mer de Glace may be seen blocs perché, moutonéed rocks, and moraine matter piled *high above the present glacier.* Again, as we journey from Chamounix to Martigny up a beautiful valley skirted by pine woods, we see the picturesque village of Argentiere opposite the Silver glacier, which descends to the valley from the magnificent Aiguille which towers above. Beyond this glacier is a great terminal moraine, forming a wooded barrier right across the valley, save on the left bank, where it is cut across by the river. This terminal moraine must have been piled up by a great glacier which once came down the valley from the Col de Balme, for it is made up chiefly of erratics derived from that district. The ice is gone, and in the place thereof we have the pine woods, the village, and the rushing river, with marks of glaciation everywhere on the sides of the valley. Beyond Argentiere, too, up the valley of Berard, on the left hand in going to the Tête Noire, are some large blocs perchés ranged in lines at a height of more than a thousand feet above the valley. These are erratics which were stranded where they now rest by a stupendous glacier which swept down from the mountains from the direction of Trient, and the mountains above the Valorcine, and of which now not a fragment of ice is left. But we will carry our investigations still farther. At the lower end of the great Italian lakes, such as Lago Maggiore, Como, Garda, and others, there are immense moraines which have been brought by extinct glaciers from the upper Alpine valleys far above the lakes. Professor Ramsay suggested that the origin of all these lakes is owing to the crushing and grinding action of enormous glaciers descending from the high Alps, which were several thousand feet higher during the intense cold of the glacial epoch than at present. At all events, it is certain that glaciers once passed over the sites these great lakes now occupy. At Isola Madre, one of the beautiful Borromean islands in the Lago Maggiore, we found the rock surface moutonéed, and stranded on it are fragments of granite and other erratics. The beautiful gardens there, with terraces of orange-trees, lemon-trees, and citrons, pomegranates, oleanders, and magnolias, are situated upon a *roche moutonée* of a great glacier which once swept down from the Alpine regions of frost. The old ice track is surrounded by the blue waters of the lake; and the nightingale sings in the grove, and the fire-fly flits, on a summer night, above the glacier's ancient bed, while miles away glisten the snows of Monte Rosa. Nor is it only by the Italian lakes we see the remains of glacier action of days long since passed away. Everywhere among the mountains and hills which surround Como and Lugano are the old ice-marks, showing that where now the sheep pastures and wild flowers are blossoming, ice masses crept for long ages over land surfaces which now nourish the fig-tree and the vine. I might give many more examples, but will only allude here to the colossal moraines of a vast glacier which, during the Glacial Epoch, swept down to Ivrea in the plains of the Po, near Turin. Hills higher than the Malverns (1,500 feet), and fifteen miles in length, are there entirely composed of moraine matter, containing enormous erratics brought by an old glacier from the high Alps between Monte Rosa and Mont Blanc. I have observed also that the ice of the Glacial Epoch extended much *farther south* than was formerly supposed. When visiting the shores of the Mediterranean, between Fréjus and Genoa, two years ago, I was struck with the moutonéed appearance of some of the rocks both inland and on the coast. This evidence, too, corresponded with that of the Mentone Caves, where the remains of the mammoth, hairy rhinoceros, marmot, and reindeer showed that the climate there must have once been very different to the present, for all these animals are northern forms, and must have migrated during the Glacial Epoch to the shores of the Mediterranean from their former home in Siberia. Still it was hazardous to speculate upon an accumulation of ice down vales now occupied by olive, orange, and lemon groves, and where even the palm-tree now flourishes. I succeeded, however, with the aid of my friend Mr. Moggridge, notwithstanding the immense amount of atmospheric *débris* with which the country is masked, in tracing glacial action among the picturesque hills which rise above Mentone. To one of these I had the pleasure of conducting my friends Sir W. Guise and Captain Price. This was below the old Castle of Agnesi, where a glaciated, polished, and grooved-rock surface had been laid bare by the quarrying of a mass of angular débris, which had protected the glaciated surface from weathering. I believe this breccia to be of the same age as that which contains the bones of the mammoth, marmot, and bison at the Mentone Caves. Higher up among several cols my friend Mr. Moggridge conducted me to localities where are erratic rocks which he recognised as belonging to the mountains which range in the direction of the Col de Tenda, and could only have been stranded where we found them by *ice*. Then when we extend our observations to our own country, it wonderfully increases our interest on a tour in North Wales, or Scotland, or among the green hills of Ireland, to mark the signs of vanished glaciers in vales around Snowdon, or Ben Nevis, or by the Lakes of Killarney. I have gathered Alpine flowers from a *roche moutonée*, or clustering beneath a bloc perché, by the waters of Llyn Lydaw (Snowdon), or the summit of Macgillicuddy's Reeks, or away among the deer forests of Sutherland or Ross. Six glaciers once flowed from the mountains of Snowdon down the valleys. The ice has vanished, but the evidence of its former existence still lingers, and few things are pleasanter during a summer's ramble than to trace the old glacier relics to their sources among the wild valleys of wild Wales. But all these proofs of the existence of *Frost action* and *Ice action*, where now there is none, take us back to the *Glacial Epoch*, during a portion of which epoch the great glaciers I have alluded to filled the valleys of Switzerland and Italy, and great erratic masses of rock were carried by land ice, and other erratics were borne by floating icebergs over the seas which then washed over large parts of Europe and North America. So intense was the cold during a portion of the Glacial Epoch that Scotland and North Wales were wrapped in ice as Greenland is now, while the ice covered every hill and valley under one continuous field of ice. Again, we learn that during this epoch there were great changes in the level of land and sea; and that it was during the submergence of large areas in Europe and America that great angular fragments of rocks were transported by icebergs far from the parent rocks to which they belong, and were deposited over wide areas of what now is dry land upraised from those glacial seas.

Such is the history of the ice-borne boulders which overlie the submerged forest of Cromer on the coast of Norfolk. They are wanderers from the rocks of Norway and Sweden, embedded in marine boulder clay; for there are associated with them *marine shells* which lived and died on the sea-bed where the erratics were stranded on the melting of the ice which bore them over the seas. The species of shells, too, testify of the cold climate which affected the seas as well as the land, when icebergs floated down to the latitude of the Norfolk shores. The "Forest-bed" itself is an ancient land surface on which, long ages ago (Pliocene or Preglacial times), there grew large forests which swept over what is now the German Ocean. And these forest lands were inhabited by great quadrupeds—elephants, hippopotami, and rhinoceri—whose remains are found in large quantities in old lake sites of that period, associated with plants and fresh-water shells. These animals belonged to a southern type of mammalia. The Glacial Epoch came on, and for long ages the forest of Cromer and its lake bed and the skeletons of the animals which had been washed into it, were sunk beneath the waters of the glacial seas and were covered up by these relics of floating ice, the erratics from the distant north. Then, in days long after, we find the forest animals succeeded by a *northern* group of animals—the mammoth, the lemming, the reindeer—which had to migrate for shelter and food from regions which had become uninhabitable through the rigour of intense frost, and where no longer could flourish even the arctic willow or the reindeer moss. And it was then that the vegetation of Great Britain became changed also. Here, then, grew the food of the reindeer, and the arctic birch flourished where many a plant had to yield before the influences of frost; and the dwarf willow, which we now find upon our highest hill summits, among glacier tracks and the groovings of extinct ice, must have grown abundantly by the caves which, in so many parts of England, yield the fossil bones of the reindeer, the mammoth, and the Irish elk.

But if this is the history of *temperate* latitudes, what has been the effect of frost and icework in *northern regions?* There was a time when Greenland and Spitzbergen were not under the dominion of frost as they are now, but when luxuriant forests grew where now sweeps the glacier, and the vine flourished upon sites now sealed by ice which no summers' sun can melt. The researches of geologists have revealed to us that this now icebound continent was, in Tertiary times (Miocene ages), a forest land, stretching away towards the Pole on the north, and to Spitzbergen on the east. Among the trees of this Greenlandic vegetation was a large Sequoia, closely allied to the great Californian pine, the Wellingtonia gigantea, which is found fossil where it grew, its roots in the soil, and around its branches, its leaves, and cones. This tree is very abundant in the lignite beds. The chestnut-tree has been determined, with its flowers and its fruit. There, too, grew the magnolia, of which both the flowers and cones have been preserved, the walnut, the plane-tree, and the vine. There were eight species of oak, the birch, the hazel, and the alder; and beneath these forest trees grew numerous ferns, while the stems of the trees were twined around by the ivy and the vine. Even the fungi on the leaves of certain trees have been detected by their spots and dots and spores, which are determinable under the microscope. Well, then, may we inquire into the *cause* of this wonderful change brought about by the frost and cold of the Glacial Epoch since Miocene times in Polar regions; as well as to the *cause* which brought about the Ice history among our own mountains and valleys of Great Britain.

For a long time geologists have endeavoured to account for this wonderful change of climate by various alterations in the positions of sea and land, elevation of high lands in Polar regions, changes in the flow of the Gulf Stream, and other geographical modifications. Sir Charles Lyell directed attention long ago to the effect which the altered positions of land and sea must have on climate in different parts of the world, and to the alteration which must affect Polar regions, or any other regions, if high mountain ranges were elevated in the place of low-lying plains, or lakes, or seas. In his great work, "The Principles of Geology," he discussed fully the effect of the Gulf Stream in modifying Arctic cold, the effect of the Fohn, or the south wind, now, in melting the ice of Alpine glaciers, and the change brought about by the elevation of the Sahara or Great Desert into land instead of sea, with various other geographical changes and conditions which do, no doubt, affect climate very considerably. But for some time past there has been a strong belief that all these phenomena combined were not sufficient to bring about that intensely cold period which for long ages held our northern hemisphere under the dominion of intense frost, and brought about such extraordinary changes of climate since the Miocene forest days of the Arctic regions. So strongly did Professor Heer feel this that, after several investigations into the history of the fossil trees and plants which formerly grew in Greenland, he said, "We are face to face with a problem whose solution must be attempted, and doubtless completed, by the astronomer." And for some years now, astronomers, mathematicians, and physicists have endeavoured to solve the problem. The possibility of an alteration in the earth's axis was maintained by the late Sir John Lubbock and M. Adhemar, and the changes which might be caused by variations in the eccentricity of the earth's orbit, with other astronomical problems, have been calculated by Mr. Croll, Mr. Stone, and other mathematicians. But I must refer those interested in such questions to Mr. Croll's work on "Climate and Time in their Geological Relations." I will only say that late geological researches do somewhat confirm Mr. Croll's views, which are derived from certain astronomical changes, and the physical conditions those changes produce on the climate of the earth. Mr. Croll believes that there have been *recurring* glacial epochs, that is to say, cold periods followed by warm periods, in both hemispheres, *throughout all geological time.* But geology has for some time past contemplated this question of *recurring* glacial periods. There is undoubtedly evidence of frost and ice action in the transported blocks which occur in conglomerates of the age of the Old Red Sandstone in Scotland. Professor Ramsay was led long ago to infer that ice action was the only way to account for the transport of large angular erratics which are found in breccias of Permian Age. Mr. Godwin Austen called attention to ice action in France during the Carboniferous Period, owing to similar phenomena presented by carboniferous conglomerates or moraines. The "flysch" of Switzerland indicates the existence of glaciers in the Swiss country during some part of the long Eocene Epoch, while during its earlier ages we know that the nummulitic ocean flowed over sites now elevated into the summits of the Diablerets and the Dent du Midi. So during the Miocene Epoch. In earlier Miocene times we find forests growing in the distant north; while in later Miocene times, or during the deposition of Upper Miocene strata, we know there were Miocene glaciers which bore down great moraines to the neighbourhood of Superga, near Turin. Lastly, the retiring of the colossal glaciers which stranded the Pierre à Bot above Neufchâtel was followed by a warm or interglacial period, with small glaciers; and then *the great glaciers came back again*, and since that have again receded to the present pigmy ice-streams now presented to our observations. These are geological phenomena not to be ignored; and without venturing to pronounce an opinion on the theories of Mr. Croll, I honestly confess many of them will, if they bear the test of criticism, prove a blessing if they enable us to solve some of the more puzzling phenomena presented by geology.

Is it likely or probable, if there was frost and ice action in Scotland and England during Devonian and Permian times, and during the Carboniferous Epoch in France, that at the same period Polar regions were warm, and free from glacial action? Again, Mr. Croll's theory as to the oscillation of the level of the waters of the oceans, caused, as he believes, by the physical effects produced by recurring glacial phenomena, appears to account for some of those changes in the level of land and water which are so difficult to account for when we have to appeal solely to local earthquake movements—earthquake movements in all kinds of places in shifting the land up and down, and down and up again. Again, recurring glacial epochs through all past time would assist us somewhat in accounting for the extermination of whole series of animals, which, as the Frost periods recurred, might have been unable to survive, and during which some species, driven southwards, might have become changed and modified. No one can tell why the mammoth and woolly rhinoceros, which once inhabited Siberia, and later on temperate Europe, in such vast herds, should have died out so as not to have left a single specimen up to historic times, or why the horse and mastodon should have perished in North America. No one can say why no trilobite lived, at all events in these latitudes, through the Permian period, or why the great Secondary reptilia, and the ammonites and scaphites, which swarmed in the cretaceous seas, should not have lived on to the days of the Tertiaries. It is very easy to say "they died out," but there must be some cause for "the dying out," and it would be satisfactory if we could arrive at some conclusion as to the reason why! Now we can easily conceive that recurring glacial periods must have a considerable effect upon the life of the period, especially when affecting latitudes where for long ages the animals and plants had been adapted to a moderate instead of an Arctic climate. Some species might never become sufficiently adapted to bear the change, however gradual, and, incapable of migration, would perish for ever, leaving only their fossil relics to testify of their former existence. Still, whether these periodical changes of climate, after the lapse of hundreds of thousands of years, have occurred or not, all will perceive what an effect Frost must have had upon that part of the globe which we inhabit, during the Glacial Epoch, and which certainly and assuredly once affected large tracts of country in Europe and America.

A large part of the northern hemisphere covered either by ice like that of Greenland, or by great snowfields and vast glaciers; great herds of animals of various kinds driven towards the south for bare life and sustenance; seas frozen where now the white sail quivers in the wind; rivers bound like iron where now the steamer dashes the waters from her prow; great cities, such as Edinburgh, whose sites were scored by the ice, or grooved by the glacier, or, like the cathedral towns of Gloucester, Bristol, and Worcester, were rolled over by the waters of iceberg-traversed seas—these are no small changes belonging to the history of the land we live in, and were the effects of *Frost*.

And I may take the opportunity of stating that the more I study glacial phenomena in various parts of Europe, the more I am impressed with the belief that the last phase of the Glacial Period, in this country at least, was a period of the return of glaciers in the Highlands, accompanied by great falls of snow on the lower hills, and over Great Britain generally. The melting of this snow, accompanied by a great summer rainfall, producing floods and fresh-water currents, was, I think, the great agent which carried down the gravelly débris which constitutes the angular detritus which is so widely spread over large areas. The same streams, too, which spread out this superficial débris no doubt also washed out of position many of the earlier marine drifts and gravels, and stranded them at a lower level. At the meeting of the British Association held at Birmingham (1865) I endeavoured to uphold these views as applied to the last and overlying drifts of the regions of

Malvern, Siluria, and Wales. Since that I have visited many parts of Europe, and an examination of the phenomena presented, in very many localities, convinces me that the sea has never risen over the land of the interior of Great Britain since the days of the mammoth and the tichorhine rhinoceros. Nay more, I believe that it was a return of glacial climate to these regions which destroyed these animals at last, for on the Malverns there is not a hollow in which the débris I allude to is quarried that does not contain remains of the long-haired elephant and rhinoceros. The old river beds of this last Glacial Period, too, are full of their bones and teeth, rivers flooded by the melting of summer snow and excessive rainfall, and along the banks of which are found marine shells which have been washed into fresh-water beds during what has not been inaptly termed "a pluvial period" by Mr. Tylor.

Every investigation I have made of late years confirms me in the opinion that there has been a return of glaciers among the mountains, and snow and frost in the plains, of Great Britain, with "a wash of many waters" in the summertime, since the days of Cave men and the Cave animals. In long-ago ages the waves of the sea rolled between the shores of the Malverns and the Cotswolds, and deposited with their currents the northern drifts and such marine gravels; but these drifts have been since much disturbed and re-aggregated by fresh-water and subaerial agency, not by the waves and currents of a returning sea. The mammoth and tichorhine rhinoceros, with Palæolithic man, were, I believe, inhabitants of Great Britain when the salt waters of the Severn straits glistened above the sites of Bristol, Gloucester, Tewkesbury, and Worcester, and lived on through all that long period which converted the Severn straits into the Severn river vale. The last of the Mammoths probably saw the configuration of this country much as it is now, but under a return of glacial and pluvial conditions, and under the dominion of rains, and snows, and frost.

HOW HERMIT CRABS GET POSSESSION OF THEIR SHELLS.

By ALEXANDER AGASSIZ.

WHILE tracing the development of one of our species of Hermit Crabs, I raised from very young stages a number of specimens till they reached the size when they need the protection of a shell for their further development. I was of course curious to see how they would act the first time when supplied with the necessary shells. For this purpose, a number of shells, some of them empty, others with the animal living, were placed in the glass dish with the young crabs. Scarcely had the shells reached the bottom before the crabs made a rush for the shells, turned them round and round, carefully examining them, invariably at the mouth, and soon a couple of the crabs decided to venture in, which they did with remarkable alacrity; and after stretching backward and forward, they settled down into their shell with immense satisfaction. The crabs who were so unfortunate as to obtain for their share living shells, remained riding round upon the mouth of their future dwelling; and on the death of the mollusk, which generally occurred soon after in captivity, commenced at once to tear out the animal, and having eaten it, proceeded to take its place within the shell.

It is of course very difficult to apply to Invertebrates many of the laws of natural selection, and thus far we know so little of the habits of most of our marine animals that it is idle to speculate upon the effect of causes which may effectually modify the life of higher animals. In the case above mentioned there is no possible connection between the embryo and the parent to account for the young having learned from the former the use of the shell and its value for his existence. We can therefore only explain the faculty of performing the act as inherited, or else as a simple mechanical act rendered necessary by the conditions of the young hermit crab. This latter seems the more probable case from the nature of the test of the hermit crab in its younger stages. While the young hermit crab soon after leaving the egg is still provided with its powerful temporary swimming feet, and while the feet of the adult can only be traced as mere rudiments behind them, the whole test of the cephalothorax and abdomen (which are symmetrical) is of considerable consistency up to the last moults preceding the stage when it seeks a shell. At that time the young are no longer symmetrical; the feet, which are now fully developed being largest on the right side, and the abdomen beginning to curve in the same direction away from the longitudinal axis. When the moult has taken place which brings them to the stage at which they need a shell, we find important changes in the two hind pairs of feet, now changed to shorter feet capable of propelling the crab in and out of the shell; we find also that all the abdominal appendages except those of the last joint are lost, but the great distinction between this stage and the one preceding it is the curling of the abdomen: its rings, so distinctly marked in the previous stages, are quite indistinct, and the test covering it is reduced to a mere film, so that the whole abdomen becomes of course very sensitive. It is therefore natural that the young crab should seek some shelter for this exposed portion of his body, and, from what I have observed, any cavity will answer the purpose; one of the young crabs having established himself most comfortably in the anterior part of the cast skin of a small isopod, which seemed to satisfy him as well as a shell, there being several empty shells at his disposal. This mechanical explanation still leaves unanswered the eagerness with which the crabs rushed for the shells, their careful examination of their openings, their taking the animal out and occupying its place; all acts which seem to require considerable intelligence, and to show remarkable forethought.—*Silliman's American Journal.*

REVIEWS.

THE HISTORY OF CREATION.*

ASSUREDLY, if by the term Creation is to be understood the making of the animal and vegetable worlds by the Creator, then the first portion of the title of the present work is slightly out of place. Of course the secondary portion of the title sufficiently explains the nature of the book; but then that in no manner affects the question. The work is essentially one which seeks to bring about the utter annihilation of the Christian faith, and a belief to the fullest extent in the doctrine of Evolution in the development of all animals in a direct line one from the other—the highest having proceeded from the lowest. Of course there is nothing new to be presented in a work like the present one. The writings of Darwin, Huxley, and Herbert Spencer have adequately enough established the doctrine of evolution in the minds of most scientific men. But something further was necessary for the introduction of this matter to the minds of the masses, and for the rendering the subject so general that the reader could grasp it in all its details. A labour of this kind was not of a simple character; it required two separate tasks to be performed by the writer—one to examine the subject in all its aspects, and the other to render his account so clear and intelligible that it would have no difficulty in entering the mind of a popular reader. How has Herr Haeckel done his portion of the task, and how have the editor and translator performed theirs? We think there cannot be a doubt—save on the part of those who are prejudiced on this point—that the author has discharged a most difficult task with conscientious skill and with marvellous ability. On some points we may think he has gone further than absolute testimony would warrant, but these are unimportant; while we cannot help admiring the honest fearlessness with which he expresses opinions which not a few of our English workers hold, but are afraid to acknowledge. In this respect he reminds us somewhat of another

* "The History of Creation; or, the Development of the Earth and its Inhabitants by the Action of Natural Causes." A Popular Exposition of the Doctrine of Evolution in general, and of that of Darwin, Goethe, and Lamarck in particular. From the German of Ernst H. Haeckel, Professor in the University of Jena. The translation revised by Professor E. R. Lankester, M.A., F.R.S., Fellow of Exeter College, Oxford. In 2 vols. London: King & Co. 1876.

German writer of the same cast of thought—Herr Louis Büchner. Haeckel, indeed, shows very early in his book the tone that he takes throughout, for we find the terse statement on the ninth page that "where faith commences science ends." Of the editor and translator we need only say that their respective duties have been well discharged.

The mode which the author adopts in laying the views of modern natural science before his readers we may understand from a survey of the contents of the several chapters in which these views are expressed. Briefly they are as follows:—Nature and Importance of the Descent-theory; Justification of this Theory according to Linnæus; Cuvier's and Agassiz's View of Creation; Theory of Development, according to Goethe, Oken, Kant, Lamarck, Lyell, and Darwin; Theory of Natural Selection, Inheritance, and Propagation; Laws of Transmission by Inheritance; Laws of Adaptation; Laws of Development of Organic Tribes and of Individuals; Development of the Universe and Earth; Spontaneous Generation, Migration, and Distribution of Organisms; Periods and Records of Creation; Pedigree of the Kingdom of Protista; Pedigree of the Vegetable Kingdom; Pedigree of Animal-plants and Worms; Pedigree of Mollusca, Star-fishes, and Articulated Animals; Pedigree of Vertebrate Animals; Pedigree of Mammals; Origin and Pedigree of Man; Migration and Distribution of Mankind; and, lastly, Objections against and Proofs of the Truth of the Theory of Descent.

These several chapters cover nearly 800 pages of print, so that it must not be said that the author has dealt lightly with his subject. But we think that, in addressing such a work to an English public, Herr Haeckel would have been well advised had he left out a good deal that he has written about the descent of animals. For it must be confessed that anything like a clear line of descent from the Amœba or its congeners to man is absolutely out of the question in the present range of science. We may construct tables of genealogy, as we see the author has often done, but we know that in the course of a few years they are certain to be broken up. Therefore, to this part of the present work we distinctly object; while, of course, we are in absolute agreement with Professor Haeckel as to the whole groundwork of his scheme. And having thus so far expressed our dissent, we may now proceed to notice some parts of this most remarkable book. One of the chapters that strikes us as especially of interest is that in which he shows up the utter fallacy of Agassiz's argument against Darwinism. Agassiz said: "Darwinism shuts out almost the whole mass of acquired knowledge in order to retain and assimilate to itself that only which may serve its doctrine"; to which Haeckel replies: "Surely this is what we may call turning the whole affair topsy-turvey. The biologist who knows the facts must be astounded at Agassiz's courage in uttering such sentences—sentences without a word of truth in them, and which he cannot himself believe! The impregnable strength of the theory of descent lies just in the fact that all the biological facts are explicable only through it, and that without it they remain unintelligible miracles. All our 'laborious knowledge' in comparative anatomy and physiology, and in embryology and palæontology, in the doctrine of the geographical and topographical distribution of organisms, &c., constitutes an irrefutable testimony to the truth of the theory of descent." This is strong language, but it certainly is not stronger than was meet for Agassiz's reckless assertion of falsehood.

Another passage in this work which strikes us as being especially interesting, is that from a letter of Darwin's to Professor Haeckel, in which the former traces out the mode in which he came to hit on his great discovery, that of Natural Selection. He says: "For some years I could not conceive how each form became so excellently adapted to its habits of life. I then began systematically to study domestic productions, and after a time saw clearly that man's selective power was the most important agent. I was prepared, from having studied the habits of animals, to appreciate the struggle for existence, and my work in geology gave me some idea of the lapse of past time. Therefore when I happened to read 'Malthus on Population' the idea of natural selection flashed on me." This fact is important to English readers, as we think it has not been mentioned in any of Mr. Darwin's own books.

On looking for further points on which to dwell in our notice of this excellent work we have come across one which is somewhat painful to read, as the supposed facts on which it rests are now admittedly mistaken ones. It is as to the Bathybius. Professor Haeckel, writing before Mr. Huxley's renunciation of this organism, describes it as being an unquestionable animal. Our readers are of course aware that Professor Huxley has some six months since completely given up the animality of this quasi-organism; it is, therefore, the more to be wondered at that Mr. Lankester should have allowed Haeckel's remarks to stand unqualified in the first volume, on p. 184, and in the second volume on p. 53, and have merely added a footnote on p. 344, vol. i., which says: "We must wait for fuller information on the subject of Bathybius at the hands of the naturalists of the *Challenger* expedition, before accepting it finally as a distinct organism." We should have expected that he would have removed the passages in which Professor Haeckel has so clearly given his assent to a doctrine whose chief advocate has torn up his brief. One explanation of this apparent neglect may be that the work had gone through the press at the time that Professor Huxley had given in his recantation, and if this be so, Mr. Lankester's note was but a foresight of the ultimate result.

After giving many examples of rudimentary organs being preserved in animals which have no purpose they can fulfil, and of the absence of organs being accounted for by natural selection—as in the case of the beetles of Maderia, which are almost all wingless—the author gives an excellent plate showing the development of the tortoise, chick, dog, and man. This shows very well to the general reader the close resemblance of these four distinct organisms at an early period of life; first, of the tortoise, dog, and man, of four weeks, and the chick of four days; and second, of the tortoise and dog, six weeks, the chick of eight days, and man of the eighth week. The subject of the wonderful relation between invertebrata and vertebrata, which was first indicated in the year 1867 by Kowalewsky's researches, has been especially dwelt on by the author of the present work, who has given us two capital plates illustrating the relation between the Ascidian Phallusia and the common Lancelet. In the first of these, the immature animals are contrasted, and the comparison shows the extraordinary relation which exists between the molluscan and vertebrate animals. Plate xiii. represents the two perfect types side by side; and though of course there are distinctive marks, yet the resemblance of the two organisms is singularly forcible. We are glad to observe too that the author draws the following conclusion to his remarks on this subject: "Of course we do not mean to say by this that vertebrate animals are derived from tunicate animals, but merely that both groups have arisen out of a common root, and that the tunicates of all the invertebrata are the nearest blood-relations of the vertebrates."

In reference to the question, from which of the quadrumana did man originate? Professor Haeckel thinks that the "*human race is a small branch of the group of Catarrhini, and has developed out of long since extinct apes of this group in the old world.*" And when on this subject he refers to Professor Huxley's remarks,* which show that man, is nearly as much as the ape, a foot-handed animal; for that various tribes of men, the Chinese boatmen, the Bengalee workman, and the negro, when climbing, use the great toe in the same manner as the monkey, and therefore that the possession of *only* a single pair of hands is not to be looked on as a character of the human race. He also points out a fact necessary to be observed by unscientific people, viz. that none of the man-like apes are to be regarded as the parents of the human race, but that the "ape-like progenitors of the human race are long since extinct." In concluding his work Professor Haeckel remarks on the desire of some who are not actually opponents of the doctrine of Descent. "They await," he says, "the sudden discovery of a human race with tails, or of a talking species of ape." But such manifestations, as the author very properly observes, would not furnish the proof desired, and unthinking persons would be provided with as satisfactory (?) arguments as they nowadays employ in hurling their defiance against all who are evolutionists.

And now we must bring our notice of this most admirable work to a close; and while we bid adieu, or rather *au revoir*, to the author, we must say a word in thanks to the publishers, who have not only done their part of the work "most excellently" well, but who have shown a courage that many would have shrunk from in bringing out this book in its English guise.

THE RECORD OF GEOLOGICAL SCIENCE.†

THIS work forms a valuable addition to geological literature, and supplies a want that must have been frequently felt by the student and worker in geological science. Although it includes only notices of works relating to geology during one year (1874), it comprises more than 2,000 separate entries of books, memoirs, maps, and sections which have appeared in that

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* "On our Knowledge of the Causes of the Phenomena of Organic Nature." By Professor Huxley, F.R.S. London: Hardwicke. 1862.

† "The Geological Record for 1874. An Account of Works on Geology, Mineralogy, and Palæontology published during the year." Edited by W. Whitaker, B.A., F.G.S. London, 1875.

period, not only in this country but throughout the world where geological research is cultivated. When it is considered the vast range of literature that had to be consulted, not always easily accessible, and in different languages, geologists cannot but feel grateful to Mr. Whitaker and his well-supported co-editors for the labour and energy they have bestowed in thus making as useful and complete as possible the first volume of the "Geological Record." The work, with the addenda, extends to nearly 400 pages, and is accompanied by a copious index. The subjects are classified under different heads—Descriptive, Statigraphical, Physical, Economical Geology, Petrology, Mineralogy, and Palæontology; and the latter (which might have been further divided) under Vertebrata, Invertebrata, and Plants. Under each heading the authors' names are arranged alphabetically, followed by the title of their works, and generally by a concise description of the contents. The maps are in the alphabetical order of places, so that the whole forms an easy and useful volume of reference.

ON RE-FORESTING IN FRANCE.*

THE severe inundations and the torrential floods which have frequently occurred in the South of France and other districts of Europe, causing extensive destruction of life and property, have drawn considerable attention to the subject, not only recently but for many years past, and have been the object of legislative measures by the Government of the former country. It is an inquiry of much importance in a scientific and economical point of view, both as regards the causes of their origin and the means to be adopted for preventing or modifying the same.

That the entire destruction of forests is one of the primary causes of torrents there can be little doubt; but besides the loss of the woods there is also the action of the torrents, in denuding the vegetable soil, in covering up the lower grounds with deposits which alter their nature, in filling up and diverting watercourses, and other injurious effects. To obviate these effects many suggestions have been advanced by various writers, especially the work of Surell, in 1841, "Étude sur les torrents des Hautes-Alpes," and to which may be traced the commencement of the works of *reboisement* and *gazonnement* which are now being carried on in the Alps, the Cevennes, and the Pyrenees, by the re-clothing the mountain-sides and brows with trees, herbage, and bush. That the remedial measures may be expensive, and the present outlay large, so also must be losses caused by these destructive floods, but the ultimate gains must be great, for it cannot be doubted that *reboisement* will be an invaluable advantage to those districts subject to torrential floods. Dr. Brown has supplied in this work much practical and useful information compiled from the numerous authorities who have treated of

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* "Reboisement in France; or Records of the Re-planting of the Alps, Cevennes, and the Pyrenees with Trees, Herbage, and Bush." Compiled by J. C. Brown, LL.D. London, 1876.

this subject, and especially what has been done in France in carrying out works of *reboisement*, with a view to preventing and arresting the destructive consequences and effects of torrents, and the results which have followed.

THE CRETACEOUS VERTEBRATA OF NORTH AMERICA.*

THIS volume is another valuable contribution to the Geological Survey of the Territories, under the direction of Dr. Hayden; and, considering how much important and interesting matter, both geological and palæontological, has been brought out by the arduous and energetic labours of the Director and his able collaborators during the progress of the survey, we cannot but hope and wish that it may be as successfully continued, by receiving the liberal support of the Government, as heretofore; not only that the geological nature of the district should be well known in its immediate economical bearings, as advancing the interest of the State, but that by the full publication of the details obtained, European geologists may be enabled to compare the lithological structure and palæontological character of the American strata with those in other areas considered to be either contemporaneous or homotaxious with them, and thus arrive at a more enlarged knowledge of the different physical conditions and the distribution of life which obtained in different parts of the earth's surface during the time assumed to belong to the same great geological period. In the latter direction the memoir on the Cretaceous Vertebrata, by Professor E. D. Cope, is very important, for palæontology lies at the very foundation of geological science; and, therefore, as a contribution towards the solution of the numerous problems involved in the geological structure of the Western Territories, as well as the unfolding of the ancient life, this work must be considered of the highest rank, and also forms a companion volume to that on the Cretaceous Flora by Professor Lesquereux, already noticed ("Pop. Scien. Rev." vol. xiv. p. 411).

Independently of its palæontological value it may be commended as a work of art, for it is illustrated by nearly sixty well-executed plates, with explanations and descriptions, together with a synopsis of the known cretaceous vetebrata of North America, which now amount to 253 species of birds, reptiles, and fishes. In this list is included numerous remains from the Fort Union beds, or lignite group, considered by Professor Cope to be of cretaceous age, but which are referred by other authors to the tertiary epoch or transition series of Hayden, from their containing a tertiary flora associated with a cretaceous fauna, thus inferring—in this area at least—there is no real physical break in the deposition of the sediments between the well-marked cretaceous and tertiary groups. With regard to the fishes of the Niobrara group, it is important and interesting to observe that nearly all

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* "Report of the United States Geological Survey of the Territories." F. V. Hayden. Vol. ii. "The Vertebrata of the Cretaceous Formations of the West." By E. D. Cope. Washington, 1875.

the genera have been also obtained from the chalk of Europe, thus pointing to the synchronism, as generally understood, between the chalk-formations of Kansas and England. Another largely developed American group are the reptiles referred by Professor Cope to his order *Pythonomorpha*, including the mososauroid forms and their allies, which according to him represent an order of reptiles distinct from any other, and present characters which ally them to both serpents and lizards. This order of reptiles attained a predominant importance during the Niobrara epoch of the cretaceous period, as is indicated by the great profusion of individual remains and specific forms. Although occurring in America, wherever the cretaceous formation appears, they are more numerously represented in Kansas than elsewhere. The seas of the American continent appeared to be the home of this order, while they were comparatively rare in those of Europe, for in the latter country only four species have been recognised. Geologists will be grateful to Professor Cope for this final but elaborate report on the vertebrate fauna of America, showing that the cretaceous ocean of the West, teeming with an abundant and vigorous life, was no less remarkable for its fishes than for its reptiles.

ANIMAL PARASITES.*

THE elder Van Beneden has passed so completely out of our recollection that we had imagined the present work was written by his son, a distinguished follower in his father's footprints. It is so many years since the late Dr. Lankester introduced to our notice in one of the Ray Society's volumes the researches of Van Beneden and Küchenmeister, that we had imagined that the Belgian professor had "gone the way of all flesh." We are delighted that our ideas were mistaken ones, and that the first experimenter on the subject of the Entozoa, and the demonstrator of the fact that the *cysticercus* of pig becomes converted into the *tænia solium* or common tape-worm of man, has now written a book on the subject of animal parasites. There are very few outside the scientific world who have any idea of the vastness of the subject of animal entozoa. There is, in point of fact, no animal which has not got its parasite. Some, indeed, have many. So that it is not at all an erroneous statement, that animal parasites are the most numerous group of beings in existence. It is clear therefore that M. Van Beneden has had a very wide field before him from which to prepare the book now under notice. Indeed, we should have thought that it would have been too vast a subject for anyone to attempt the treatment of in a single volume like that before us. However, there are two modes of dealing with the subject, the scientific and the popular; in the one of course the writer must treat at length, fully and completely, with each animal under discussion; in the other he can pass in a sketchy manner along the field of his discourse, and can touch lightly on the more complex subjects, or enlarge

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* "Animal Parasites and Messmates." By P. J. Van Beneden, Professor of the University of Louvain, Correspondent of the Institute of France. With 83 Illustrations. London: King & Co. 1876.

on those best suited to the public mind. It is the latter view which M. Van Beneden has taken. And in our opinion the choice has been a perfectly wise one. For it must be remembered that the book is not a strictly scientific one, but, on the contrary, is intended for the man who has scientific tastes without scientific knowledge. Therefore the whole style and tone of the present work are in adaptation to the wants of the general reader. If there is one defect in M. Van Beneden's observations it is that he seeks in some cases to be pleasant in style at the expense of conciseness of language and clearness of thought.

In the first instance we must take exception to the author's division of parasites, which seems to us to be a perfectly artificial one, and one too which, however well it may look on paper, does not hold water as a practical mode of grouping. He divides all parasitic animals into *Messmates*, *Mutualists*, and *Parasites*, and the last he groups as parasites free during their whole life, parasites free while young, parasites free when old, parasites that migrate and undergo metamorphoses, and lastly parasites during their whole life. Under these several headings he supplies a vast amount of information, occasionally illustrated—but it must be confessed imperfectly—in which he gives an excellent and popular account of the singular vagaries of the various parasites. Talking of the wanderings of the individuals which make up an entire cestoid worm, he says that recently Herr Leuckart, in concert with M. Mecznikow, has discovered "transmigrations of worms accompanied by changes of sex; that is to say, they have seen nematodes, the parasites of the lungs of the frog, always female or hermaphrodite, produce individuals of the two sexes which do not resemble their mother, and whose habitual abode is not within the lungs of the frog, but in damp earth. In other words, let us imagine a mother born a widow, who cannot exist without the assistance of others, producing boys and girls able to provide for themselves. The mother is parasitical and viviparous, her daughters are during the whole of their life free and oviparous."

M. Van Beneden might well have given us more information on the subject of the *Gregarinæ* than he has furnished; the paragraphs supplied being extremely brief and not by any means exhaustive. However, with this and one or two other instances of neglect, the author has performed a difficult task with admirable skill. He has told us many wonderful and truthful tales, and has brought so much humour to bear on the subject, that he has made many unusually dull questions sparkle with the wit and vivacity with which he has surrounded them.

FIRST BOOK OF ZOOLOGY.*

THERE is in this little work a considerable amount of originality exhibited, both in composition and illustrations. The book too is luxuriously got up as to type and paper. With these remarks our friendly criticism

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* "First Book of Zoology." By E. S. Morse, Ph.D. Henry S. King & Co. 1876.

ceases. There is an absence of plan in the construction of the work. Details oft of no importance are too much attended to. Withal the book is styled a work on zoology, yet the vertebrata occupy only about ten or twelve pages. Then there is an utter absence of anything like classification. Finally, the author's list of works to be referred to shows an utter misconception of the nature of the wants of the student. We cannot recommend the work to any but the dilettante teacher of zoology.

POPULAR NATURAL HISTORY.*

OF the two little books that we have before us we take that which is written by the woman to be the best, as it certainly is the most original. Indeed, the "Dwellers in our Gardens" is in many respects a clever book, in which many points in natural history are exceedingly well told in pithy language, and withal in a style which is quite commensurate with the intelligence of the class to which it is addressed. It is specially interesting from the fact that it possesses as a frontispiece a plate which is apparently borrowed from the "Curiosities of Entomology," and which shows us at a glance several remarkable instances of insect disguises. And by insect disguises we mean some of those resemblances of animals to plants which serve to preserve the former, by the natural preservation which they afford to them from their enemies. Many of these curious facts in natural history have been indicated by Darwin, Wallace, Belt, Bates, and others, and in the works of few of these distinguished writers are the curious facts of animal disguises better illustrated than in Sara Wood's little volume. Her several chapters are all taken up with the ordinary animal occupants of our country gardens. But there are little bits here and there that show us the writer's observations as a naturalist. This is especially to be seen in the remarks on the spinnerets of the spider, in which the structure of the web and apparatus for its manufacture are very fully and intelligently explained. In the chapter which is devoted to the lepidoptera we find a full account of the microscopic scales of the butterfly, and of the numerously curious forms of the eggs in the lepidoptera. The other chapters on bees, aphides, birds, and frogs are likewise interesting; and, together with the charming coloured plate of birds, birds' eggs, moths, and butterflies, form a volume of pleasant and useful material for the young. Indeed, we have never seen a better coloured plate than that of the golden-crested wren, which faces p. 107 of this excellent little work.

Mr. Houghton's book, which is issued by the same house as the above, and which is got up in a somewhat similar style as regards paper, print, and illustration, is not so original, but is by no means a bad essay on the subject.

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* "The Dwellers in our Gardens: their Lives and Works." By Sara Wood. London: Groombridge. 1875.

"Sketches of British Insects: a Handbook for Beginners in the Study of Entomology." By the Rev. W. Houghton, M.A., F.L.S. London: Groombridge. 1875.

Of course there is nothing new in its pages, but the facts of entomology are pleasantly put, and the introduction on the anatomy of the animals is not by any means a bad sketch of insect structure. As a book on entomology, we think it a very excellent work to place in the hands of a beginner.

FOOD AND ITS ADULTERATIONS.*

WITHIN the past ten years the subject of adulteration of food has acquired an immense importance, from the fact that it has been during that time possible to detect any infringement of the law, and equally possible to punish—and that most severely—those who have been guilty of the act of adulteration. During that period various books on the subject of the detection of impurities in food have been published, among the first of which we should certainly place the work on hygiene of the late Dr. Parkes, of Netley. Indeed, we know of no other writer who treated generally on the subject, with the exception of the author of the present volume, who published, eighteen years ago, a work entitled "Adulterations Detected in Food and Medicine." Now, however, Dr. Hassall has again come to our assistance, and has brought out a new edition of this essay; and he has so modified his original remarks, and has added so largely to the contents, that we think he is perfectly justified in issuing the work under a new title, that of "Food: its Adulterations and the Methods of their Detection." To do anything like a fair review of this work would be impossible in anything less than a couple of sheets of matter. And that this is so will be at once evident when we state that it consists of nearly 900 8vo. pages of closely-printed matter. We shall therefore only touch on some two or three points, after giving a sketch of the book's general plan. Dr. Hassall's work is divided among fifty different chapters, as follows:—Firstly, we are treated to some observations on the subject of food in regard to its functions and quantity, and then we have a chapter devoted to the different modes of preserving food. After this come the following chapters, in each of which the author deals fully with his subject, showing us the characters of both the normal and adulterated article, and offers illustrations most of which are reliable, though in one or two instances they are ideal—that is to say, that *one* illustration represents every conceivable form of abnormal variation: water, tea, coffee, chicory, cocoa, sugar, honey, flour, bread, oatmeal, arrowroot, sago, tapioca, proprietary alimentary preparations, milk, butter, cheese, lard, isinglass, gelatine, unwholesome and diseased meat, potted meats and fish, anchovies, bottled fruits and vegetables, tinned vegetables, jellies and preserves, mustard, pepper, cayenne, spices, curry-powders, turmeric, liquorice, annatto, vinegar, pickles, lemon and lime-juice, sauces, aërated waters, malt beverages, cider and perry, wine, and, lastly, spirituous liquors. Then,

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* "Food: Its Adulterations and the Methods for their Detection." By Arthur Hill Hassall, M.D., M.R.C.P., &c. &c. Illustrated by upwards of 200 wood engravings. London: Longmans. 1876.

after the discussion of these several chapters, comes one on the utensils used in the preparation and storage of food, and on various methods of detection of adulterants. These are followed by a general summary of adulteration; and last, but by no means least, comes a chapter useful to both the medical officer of health and the chemical analyst, and that is the Act for the sale of food and drugs, which was issued in 1875.

Out of this enormous list of subjects it is hard to choose any, for all appear of almost equal import. However, we may select a few, and first of water. This is a form of food which all, whether high or low, have to partake of, and it is therefore of the utmost importance that we should have the means of detecting the presence of impurity. Indeed, it is the more so from the fact that it is now believed to be the medium by which we take into our bodies the contagions of cholera, typhoid fever, and possibly also scarlatina. We are glad to see, therefore, that Dr. Hassall has given ample space to the treatment of this subject. His remarks cover no less than 77 pages, and deal with every possible question connected with the purity and impurity of almost every conceivable form of water. From these we select his remarks on the question of filtration. For it is by the filter alone that people generally have any control over their drinking-water, and it is of great importance that attention should be paid to the following statements:—

"Of course the powers of all filters are limited, and they speedily become spoiled when too much work is thrown upon them at one particular time—that is to say, when water containing a large quantity of organic matter, say, six or eight grains per gallon, is rapidly passed through them. In this case the requisite time is not afforded for the due action of the filters, which become simply clogged; but when water containing only a moderate amount of impurity, as one grain per gallon, is passed through, then the action of the better filters, especially those containing charcoal, is not only satisfactory but continuous." In regard to the mode of cleaning filters, which should be done often, contrary to the usual practice of keeping them in perpetual action from year's end to year's end, Dr. Hassall quotes as follows from Dr. Parkes' able treatise: "Every two or three months (according to the kind of water) four to six ounces of the pharmacopœial solution of potassium permanganate, or twenty to thirty grains of the solid permanganate in a quart of distilled water, and ten drops of strong sulphuric acid, should be poured through, and subsequently a quarter to half an ounce of pure hydrochloric acid in two to four gallons of water. This will aid the action of the permanganate, and assists in dissolving manganic oxide and calcium carbonate. Three gallons of distilled or good rain-water should be then poured through, and the filter is fit again for use." We think it a pity that Dr. Hassall reproduced his old cuts representing the fresh-water animals, as they convey incorrect ideas at the present time. And we would also allude to the objectionable analysis of Messrs. Allsopp's beer, for which Dr. Hassall has had to offer a public apology.

Under the head of milk and its adulterations we find some very useful practical hints, as well as various methods of analysis. But we are not in agreement with the author in thinking that sheep's brains are used in the adulteration of milk. However, he gives an illustration showing the appearance of milk which does possess this form of adulterant. We are surprised to find how frequently anchovies are adulterated; in fact, how very seldom we can obtain them pure and undefiled. From an examination made by Dr. Hassall it seems that of twenty-eight samples of anchovies, "seven of the samples consisted entirely of Dutch fish, two consisting of a mixture of Dutch fish and anchovies, and that the brine in twenty-three of the samples was charged with either *bole Armenian* or *Venetian red*." The last subject to which we shall refer in this notice is Dr. Hassall's observations on the subject of beer. Of this drink he records the following different kinds of adulterants: water, cane-sugar, liquorice, burnt sugar, gentian, wormwood, quassia, calumba root—detectable by Mr. Sorby's spectroscope—chiretta, bitter orange-peel, camomile, picric acid, picrotoxin, nux vomica or strychnine, opium, tobacco, ginger, capsicum, sulphate of iron, and alum.

The classified list which the author places at the end of the work will be found highly useful by the analyst. In conclusion, we may say that we regard this work as in every way a most thoroughly satisfactory one, and without which the analyst's library would be most seriously deficient. We have only to regret that the author has adopted the system of inducing persons to advertise their goods at the conclusion of his volume. The practice is somewhat novel, and smells strongly of the shop.

NATURAL SCIENCE: WHAT IT IS.*

WHEN we first took up this book we exclaimed, with a feeling somewhat of contempt, "Here is another Mrs. Somerville!" But before we had read the first half-dozen pages we laid it down with an expression of admiration of the very wonderful powers of the writer. And our opinion has increased in intensity as we have gone on through the work, till we at length have come to the conclusion that it is a book worthy of being ranked with Whewell's "History of the Inductive Sciences;" and it is one which should be first placed in the hands of everyone who proposes to become a student of Natural Sciences, and it would be well if it were adopted as a standard volume in all our schools over which the School Boards have authority. The writer has traced in a most clear style the progress of Natural Science—including, of course, a certain amount of Physical Science—from the times of Pythagoras and the other Greek philosophers, before the Christian epoch, down through the period of the Ptolemys; then through the time when science was unknown, save to the Arabs, who first introduced algebra to the world, to the period of the so-called Middle Ages, when it began to be pursued by some few Europeans. Beginning then at the sixteenth century, she has given us an admirable account of the progress made in that age, and in the seventeenth and

* "A Short History of Natural Science, and of the Progress of Discovery from the Time of the Greeks to the Present Day; for the Use of Schools and Young Persons." By A. B. Buckley. With Illustrations. London: John Murray. 1876.

eighteenth of the Christian era. Next comes the nineteenth century, the especially scientific age, and this occupies far more than one-third of the entire volume. Of the plan of the book we have only to speak in the most favourable terms. And of the author's style we cannot speak in terms which are too warmly praiseworthy. Let us take a couple of examples; one from the early portion of the book, and another having to do more with our own time. And first let us see how she explains the well-known experiment of Archimedes when he was discovering specific gravity. It is not an easy point to explain lucidly to one who is totally ignorant of science, yet we think Miss Buckley has been wonderfully successful. Speaking of Archimedes' well-known excitement and his cry of "*Eureka, Eureka!*" * she asks, "What had he found? He had discovered that any solid body put into a vessel of water displaces its own bulk of water, and therefore, if the sides of the vessel are high enough to prevent it running over, the water will rise to a certain height. He now got one ball of gold and another of silver, each weighing exactly the same as the crown. Of course the balls were not the same size, because silver is lighter than gold, and so it takes more of it to make the same weight. He first put the gold into a basin of water, and marked on the side of the vessel the height to which the water rose. Next, taking out the gold, he put in the silver ball, which, though it weighed the same, yet, being larger, made the water rise higher; and this height he also marked. Lastly, he took out the silver ball and put in the crown. Now, if the crown had been pure gold the water would have risen only up to the mark of the gold ball; but it rose higher, and stood between the gold and silver mark, showing that silver had been mixed with it, making it more bulky. This was the first attempt to measure the *specific gravity* of different substances."

We have chosen the above passage as an illustration of the author's admirably clear and simple style, and of her success in rendering what is not the simplest matter in the world perfectly intelligible to an average reader. And it is the same fashion in which she deals with many other and more complex problems. But what we most admire about the book is the utter absence of the "goody-goody" tone, which most women and not a few men feel it incumbent on them to introduce when attempting to explain any fact in science to those whom they consider to be beneath them in intellectual knowledge.

We shall only select another passage from Miss Buckley's book, though had we space we should quote it more abundantly. It is that in which she explains to her readers—supposed to be children—the effects of natural selection. Now, this is not an easy matter even for an adult to explain, for we doubt not there are many even among our better educated friends who would feel themselves utterly "at sea" if such a question were proposed to them. Hear how the authoress explains the matter. "Mr. Huxley tells us that a single plant producing fifty seeds a year would, if unchecked,

* It was, of course, in reference to the question put to him by the king, if he could find out whether the jewellers had, in making the crown, kept back some of the gold, and supplied its weight with some other metal.

cover the whole globe in nine years, and leave no room for other plants. It is clear, therefore, that out of these numbers millions must die young, and it is only the most fitted in every way that can live and multiply." Mr. Darwin "tells us that the heartsease and the Dutch clover, two common plants, only form their seeds when the pollen is carried from flower to flower by insects. Humble-bees are the only insects which visit these flowers; therefore if the humble-bees were destroyed in England there would be no heartsease cr Dutch clover. Now, the common field-mouse destroys the nests of the humble-bee ; so that if there are many field-mice the bees will be rare, and therefore the heartsease and clover will not flourish. But again, near the villages there are very few field-mice, and this is because the cats come out into the fields and eat them; so that where there are many cats there are few mice and many bees, and plenty of heartsease and Dutch clover. Where there are few cats, on the contrary, the mice flourish, the bees are destroyed, and the plants cease to bear seeds and to multiply." We can quote no further, but we may state that the writer goes on to show that the scheme may be extended still more by imagining that certain mice are, through their peculiar odour, disliked by cats, and thus a new race arises which destroys the bees, and thus destroys the clover and heartsease. and then she goes on to consider the possibility of a new race of plants coming into existence through the influence of moths, which would by fertilising those plants with peculiar drooping petals enable those only to be propagated.

The matter we have taken from Miss Buckley's book is adequate to prove that not only does she possess the ability to teach, but that she also has the knowledge from which to draw her various lessons. And in concluding this very imperfect notice of her labours we must express the hope that, notwithstanding Sir J. Lubbock's recent failure in the House of Commons to modify the scheme of the School Board, some effort may be made to introduce her volume very largely to the educational classes.

THE GREAT DIVIDE.*

THE Earl of Dunraven has given us a clever sketch of his excursion to the Yellowstone territory of North America—to that portion of the most majestic scenery of the United States which has recently been converted into public property by a bill passed in the Senate about four years since. The term *Great Divide* he has chosen because he considers that the line of mountains of which this Yellowstone *Park* (as it is, we think, somewhat absurdly styled) forms part, is a natural division of the continent into two portions, an eastern and a western. He has thus termed his book "The Great Divide," an expression, we opine, that very few of his readers will guess the significance of. The Earl has done wisely, in presenting his adventures to

* "The Great Divide: Travels in the Upper Yellowstone in the Summer of 1874." By the Earl of Dunraven. With Illustrations by Valentine W. Bromley. London: Chatto & Windus. 1876.

the public, to avoid anything like exaggeration. Indeed, he himself states that he has met with nothing extraordinary in the course of his explorations. No tomahawking or hairbreadth escapes from Indians are to be found in these pages. Indeed, on the contrary, with two or three exceptions, the pages of the volume before us do not record anything that might not have occurred to a person travelling in the Austro-Italian Tyrol. Though, of course, had the Earl of Dunraven taken a different route—which at the time was blocked, owing to disputes between the American Government and the Indians—he might not have had an opportunity of discoursing so pleasantly on the subject of the Great Divide. Still his account is full of interesting matter, and in no respect is it so good as in description of scenery. The author appears to have a wonderful power of sketching out in words the scenes which passed before him. And in no case is this better shown than in his graphic description of the wondrous view obtained from Mount Washbourne, the peak *par excellence* of the Yellowstone mountains. "From it has been traced out the geography of the country. The main divisions, the great centres of trade, together with the natural features that sway the fates of men and nations, radiate thence; and by a citizen of the United States the spot should be regarded as sacred ground. From it he can overlook the sources of the Yellowstone, the Wind River, and the Missouri, and of the Snake and Green Rivers, principal tributaries the one of the Columbia, the other of the Colorado. These waters flow through every variety of climate, past the dwellings of savage hordes and civilised nations, through thousands of miles of unbroken solitude, and through the most populous haunts of mercantile mankind; now shaded by the great pine-trees of the forest, again shadowed by tall factory chimneys; here clean and undefiled from the hand of Nature, there turbid and contaminated by contact with man; and from Mount Washbourne I believe that the head-waters can be seen of mightier rivers—rivers passing through more populous cities, through the hunting-grounds of more wild tribes, through greater deserts, through countries more rankly fertile, through places more uncivilised and savage, by scenes stranger and more varied, than can be viewed from any other point on the surface of this earth."

The engravings are numerous, but in most instances they lack the interest that would have arisen had they represented the country rather than the explorers themselves. Still they are not without cleverness both of design and execution. In some cases, too, the author's style is coarse, and we had almost said ungentlemanly. For instance, in describing the habits of a female Indian he observes: "For his helpmate is reserved a smaller but more vivacious species of game, in the pursuit and capture of which she must take great delight, to judge by the interest portrayed in this case on the countenance of the lady, as with unerring eye and unfaltering hand she through the thick tangles of her husband's hair hotly pressed the bounding fugitive, or like the relentless blood-hound surely tracked to his lair the slow-crawling and unmentionable one." However, the defects in the volume are small; and though the book can hardly be termed scientific, still it does contain some references to geology and chemistry, and it is an honest and clever account of the author's experiences of men and things.

CAROLINE HERSCHEL.*

THERE are few who have any interest in astronomy who will not be delighted to read this charming life, told as it is nearly in the words of ts subject. It is, in fact, almost an autobiography, for the few remarks that Mrs. John Herschel has added do little more than serve to bind the various letters and statements of Caroline Herschel into something like a continuous record. What a wonderful old and young woman she appears to have been, and how marvellously she was devoted to her brother William, who was first musician and then astronomer! She, says Mrs. Herschel, "had been his helper and assistant in the days when he was a leading musician; she became his helper and assistant when he gave himself up to astronomy." We think that Mrs. John Herschel has done well to have handed us down the life of her distinguished ancestor, as well because her subject deserves a biography as because that biography is full of interesting facts to the lovers of the stars. We had almost forgotten to mention that the frontispiece is an excellent portrait of the old lady at the advanced age of ninety-two.

ARCTIC YACHTING.†

WITH the assistance of Dr. W. S. Livesay, who has done the editing and illustration of this work, Mr. Lamont has given us an instructive sketch of his five voyages of sport and discovery in the districts of Spitzbergen and Nova Zembla. There is not much to be said about the natural history of the book, save that some capital illustrations are given of the walrus, the bear, and the reindeer; but as a work of travel the work has many points of interest, though not of novelty. Still it is a volume which from its illustrations alone, which are both numerous and good, must have an interest for all, and from its matter, which is told in a graphic manner, must especially excite those who care about adventure. In some cases the author was in extreme danger, and many incidents he records remind us of our brave sailors who are at the present moment exploring the polar regions. Readers who are members of the Royal Geographical Society should purchase this volume.

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* "Memoir and Correspondence of Caroline Herschel." By Mrs. J. Herschel. With Portraits. London: J. Murray. 1876.

† "Yachting in the Arctic Seas; or, Notes of Five Voyages of Sport and Discovery in the Neighbourhood of Spitzbergen and Novaya Zemlya. By J. Lamont, F.G.S. Edited and Illustrated by W. Livesay, M.D. London: Chatto and Windus. 1876.

THE THREE HEAVENS.*

THOSE who like their science worked up with a certain amount of Scripture references will take an especial delight in the work which is entitled the "Three Heavens." It is the reprint of a series of essays which have appeared in a periodical termed the "Sunday Magazine." The writer, who is a son of the eminent Irish surgeon, Sir P. Crampton, has a very perfect knowledge of his subject, and he writes well and illustrates his remarks very happily. We do not know that we can say anything more in praise of his book; whilst of his many theories in regard to miraculous clouds, *moral* thunderstorms, Scriptural evidence of heaven, and unnatural separation of science and religion, we suppose we had best leave the reader to form his own opinion.

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* "The Three Heavens." By the Rev. Josiah Crampton, M.A., Rector of Killesher. London: W. Hunt & Co. 1876.

SCIENTIFIC SUMMARY.

ASTRONOMY.

A WINTER of such peculiar and continuous obscurity could not be expected to testify to much astronomical progress as far as observation is concerned. From Mr. Gledhill's analysis of the weather, recently communicated to the "Astronomical Register," it appears that at Mr. Crossley's observatory, near Halifax, there were 2 very fine, 9 good, 22 fair, and 21 bad nights during the year 1875; and matters have certainly not been mending since in England. We can only hope that observers in other parts of the world may have been more favoured.

The Sun has continued in a very undisturbed condition, though one large spot, visible to the naked eye, was reported in February, which may be the precursor of renewed activity. From an ingenious method of registering isothermals on the sun's disc by the blackening of the double iodide of copper and mercury, Professor Mayer has been led to the conclusion that areas of uniform temperature exist on the sun's surface, the position of which is subject to continual variation. Professor Langley thinks that the light of the dark nuclei of solar spots equals at least 5,000 times that of the full moon.

Venus.—This beautiful planet has been gradually increasing in brilliancy and altitude, and is now in a very good position for observation. On several occasions during the present season Mr. Dennett has distinctly traced spots on her disc with a 2¼-inch achromatic; and on Feb. 29 the present writer was able to confirm their existence with a powerful silvered-glass reflector; though they were so faint that they would have escaped any cursory examination, and could not have been delineated. It is very desirable that more attention should be paid to this body, so conspicuous, so near, and yet so little known.

The Moon.—A new and important work on our satellite may be expected from Mr. Neison, who has for some time paid much attention to selenography. He has taken very accurate micrometrical measures of the position of 35 points on the lunar surface, with a view to rectify the determination of the equator and first meridian. These include 4 previously measured less correctly by Lohrmann and 4 by Mädler.

Mars.—Five drawings, taken in June and Aug. 1875, by Holden, with the 26-inch Washington achromatic, power 400, have been presented to the

Royal Astronomical Society. Some small white projections from the S. limb, occasionally seen, were not sufficiently steady for delineation.

The Minor Planets.—Since our last notice, No. 157 has been discovered by Borelly, at Marseilles, Dec. 1, 1875; No. 158 by Knorre, at Berlin, Jan. 4, 1876; No. 159 by Paul Henry, at Paris, Jan. 26; No. 160, in America, Feb. 25. Hilda, No. 153, is found to have a very long period of 7·85 years, and to approach Jupiter as nearly as 0·564 of the earth's mean distance from the sun.

Jupiter.—Eleven drawings, taken with the same telescope and observer as those of Mars, during June and July 1875, have been presented to the Royal Astronomical Society. That body has appointed a committee, consisting of Huggins, Knobel, Lord Lindsay, Lohse, Ranyard, Lord Rosse, Terby, and Webb, to obtain as extensive a series as possible of observations and drawings of this planet. A mean of the measures of Struve at Dorpat and Engelmann at Leipzig gives for the respective diameters of the satellites in seconds and English miles (for solar parallax 8″·875) I. 1″·048; 2,435.—II. 0″·911; 2,115.—III. 1″·513; 3,515.—IV. 1″·278; 2,970.

Neptune.—Professor Newcomb finds, with the Washington achromatic, the orbit of the satellite sensibly circular; inclination 121·°7. No trace of another satellite. Mass of primary, $\frac{1}{19,400}$.

Comets.—No new comet appeared in 1875; but the return of Encke's was closely observed. Von Asten finds in it an undoubted acceleration, not due to a resisting medium, but, as Bessel thought, to the phenomena of "outstreaming" from the nucleus. Ranyard has pointed out the evidence of a duplex structure in the comet of Coggia, 1874. With observed, July 8, a remarkable oscillatory motion in the fan-shaped jet. Schmidt assigns to its nucleus a mean diameter of 290 miles.

Double and Variable Stars.—The companion of Sirius has been repeatedly measured at Washington since 1873.—40 *Eridani.* This is an interesting and suggestive group. It is noteworthy in the first place for the large proper motion of the *lucida*, amounting, according to Mädler, to − 2″·188 in R.A., and − 3″·470 in Decl. Then for its physical connection, as proved by community of spatial displacement, with a 9·5 mag. companion, which nevertheless has kept its relative position since 1783: then for the binary character of the companion, a pair in undoubted motion: and finally for its possible connection with two other small stars in the vicinity. These were measured by Winnecke in 1864 at 75″·85 and 89″·45; and fresh measures during this winter may prove decisive, as those distances, on the supposition of mere optical juxtaposition, would be respectively varied to 41″·1 and 107″·3.—44 *Boötis.* Dr. Doberck at Markree has found the period of this star 261·12 years, with a semi-axis major of 3·″093. The same computer gives respective periods of 1578·33 and 349·1 years to ζ *Aquarii*, and 36 *Andromedæ.* For η *Cassiopeiæ* he gives 222·435 years, with a semi-axis more than twice as great as that of the orbit of Neptune. For the very difficult pair ω *Leonis* he finds a period of 107·62 years.—γ *Coronæ Australis* has, according to Schiaparelli, a period of 55·582 years. Burnham has found that his pair β *Leporis* is in very rapid motion, 12° *per annum.*—μ *Cassiopeiæ.* It appears from Smyth's "Cycle" that there was in his time a 6 mag. star just 18′ S. of μ, of a decided though not deep red. This was not observed by Piazzi, and cannot now be found: probably a variable of long or irregular period.—The period of *Algol*, which had been diminishing since 1782, at first slowly, and then more rapidly, after remaining stationary for a time, seems to be slowly decreasing again. There is a suspicion of variation in its *comes*, first discovered by Schröter, Oct. 12, 1787, who found it sometimes quite invisible in the finest weather.—A ruby star, Schjellerup 238, near 4 *Capricorni*, considered by Sir J. Herschel perhaps the finest of that class, has been noticed by Gore in the Punjaub to be variable by at least two magnitudes.—Schmidt finds the period of Hind's red star R *Leporis* 438 d. 230 increase, 208 decreasing: he supposes the colour to be diminishing in intensity (?). This star, the most beautiful of its kind visible in England, escaped the notice of Sir J. Herschel.—The Astronomer Royal has announced that a new star-catalogue is in preparation, from the Greenwich observations of eight years. Stellar spectroscopy has been vigorously prosecuted there, especially with respect to the approach or recession of stars, as alleged by Huggins. The Greenwich observations were at first discordant, but had grown more consistent and confirmatory of Huggins's results; the obstacles, however, were extremely great, especially from the difficulty of obtaining true plane surfaces—The lamented D'Arrest published in the "Astronomische Nachrichten," a little before his death, a continuation of his most interesting spectroscopic examination of about 11,000 stars. He verifies Secchi's fourfold classification, and his last series consists chiefly of the third type, which he finds on the whole strikingly accordant in character. The want of agreement in many cases between ocular and spectroscopic estimates of colour deserves attention.—*The Pleiades.* Wolf, at Paris, has measured 53 stars in this group, and detects 499 within $135' \times 90'$ around η *Tauri*. *Merope* and *Atlas*, with some of the smaller stars, he considers decidedly variable, and probably *Maia*.—The singular nebula near *Merope* has been watched by Wolf and Stephan, who consider its variability confirmed, as it was seen by the former in March 1874, but missed by both from Nov. 1874 to the end of Feb. 1875.—Tempel gives its dimensions $35' \times 20'$. (It has been feebly visible during the recent winter.)—The *Ring Nebula* in Lyra has been carefully examined and measured by Holden with a power of 400 on the 26-inch object-glass of the Washington observatory, and a drawing sent to the Royal Astronomical Society. Nothing new seems to have been discovered. Some brighter patches were seen on the annulus, but similar ones had been discovered and found variable by Schröter and Harding in 1797 and 1798. A minute star was also seen by Holden, near the centre, which had been noticed by Secchi and Schultz and at one time by Von Hahn, with a 20-foot reflector, in the beginning of this century, but was missed by D'Arrest. Sir W. Herschel (and Chacornac) supposed the annulus resolvable. Lord Rosse's nebulous wisps, extending outwards in all directions, were not seen at Washington, and the ends of the minor axis were reversed in sharpness at the two observatories. —30 *Doradûs.* Burton, at Rodriguez, with 12-in. silvered mirror, found some but not very essential variation from Herschel's drawing. Its spectrum was strong and continuous, crossed by a bright line. The nebula round η *Argûs* he thinks not much changed. Ellery, at Melbourne, with a telescope much more differing from that of Herschel, finds decided and progressive change in both nebulæ, most remarkably in that of η *Argûs*; the smaller class of nebulæ are also constantly changing from year to year, giving new life to such observations. The bright yellow line and occasional red line seen in the spectrum of η *Argûs* for five or six weeks, four or five years ago, have never been since seen.—The definition of the Melbourne reflector has been wonderfully improved by better adjustment, and it now seems likely to answer every expectation.—The great Paris reflector is of the Newtonian, not, as was stated in our last number, of the Lemairean or "front view" construction.

BOTANY AND VEGETABLE PHYSIOLOGY.

The Formation of Starch in Chlorophyll-grains (in the green parts of plants), under the action of light, was made out by Sachs ten or twelve years ago, and has been confirmed, and the conditions recently studied in various ways by Famintzin, Kraus, and Godlewski. It is maintained, says "Silliman's American Journal," if we rightly understand, that this starch is directly formed from carbonic acid under the action of light, *i.e.* is a primary product of assimilation. Böhm, of Vienna, who contested this, but whose view according to Sachs "has already been sufficiently refuted," has returned to the subject, with new observations and experiments, and now asserts, upon reasons stated, that "The starch appearing in the seed-leaves of plantlets of cress radish and flax, is not a direct assimilation-product, formed by the immediate decomposition of carbonic acid, but a transformation-product from a reserve of nutriment already present." One would surely expect that the primary product of assimilation of carbonic acid and water would be an organisable plasma from which the starch-grains in question are constituted, not an organic structure such as a starch-grain is. It is not clear that there is much real difference between the two views, at least between those of Sachs and Böhm. For since starch, as the former explains, is formed and reformed in various parts of the plant and away from light and from chlorophyll, and in the leaf is allowed to be only one of the products of assimilation, Sachs' assertion that starch in chlorophyll-grains is "a product of assimilation," and Böhm's that it is not "a direct assimilation-product," are by no means in necessary contradiction. Nor is there apparent reason for supposing that a starch-grain in a potato-leaf is differently originated from one in a potato, except that the former is constructed of new-formed material.

The Plants of Guadaloupe Island.—At a recent meeting of the American Academy of Arts and Sciences, Mr. S. Watson presented a paper on a collection of plants recently made by Dr. E. Palmer, in Guadaloupe Island, off Lower California. It was found to contain 119 species, including twenty-one belonging to the higher cryptogamic orders, besides a dozen of probably recent introduction. The number of new species is twenty-two, with two new genera, almost all nearly allied to Californian species and genera. Of those before known, all are Californian, and most have a wide range through that State. The flora of Mexico is scarcely represented, but on the other hand some fresh indications are found of a connection between our western flora and that of South America.

Hydrostatic Pressure in Vegetable Cells.—In a recent number of the "Botanische Zeitung," there is an abstract of a communication made by Professor Pfeffer to the botanical section of the Association of German Naturalists and Physicians, at Graz, 1875, on the subject of the origin of high hydrostatic pressure in vegetable cells. This pressure, amounting sometimes to several atmospheres, even where there is only slight concentration of the fluid contents of the cells, led him, on theoretical grounds, to refer it to the molecular condition of the primordial utricle. This conclusion was confirmed by experiment. With contraction of the molecular interspaces resistance to filtration increases, and likewise the pressure which is brought about endosmotically.

How long do Seeds preserve their Vitality?—This question, which has been often answered before, has been taken up by Herr Hoffmann, who, in a series of papers in the "Botanische Zeitung," gives his view of the matter, and describes experiments with *löss*, a diluvial earth found in the valley of the Rhine. When the railroad station Monsheim (at Worms) was built, the earth was dug away to a depth of twelve feet. Some of the *löss* was taken with necessary precautions, and securely sealed until the following spring (1865). In May twenty-four flower pots were half-filled with manure which had been heated in order to destroy any seeds present, and on this substratum some of the *löss* was placed, leaving an air-space above, of two inches, and each pot was covered by a glass disc which had a bit of wood under one edge to allow access of air. The surface of the *löss* soon had plenty of ferns and mosses, just like those which are so abundant in all greenhouses. A few phænogamic plants came up; four which could not be determined accurately were supposed to be *Vaccinium myrtillus*, a second, a *Chrysanthemum Leucanthemum;* afterwards a third came up, a *Galium*, and finally an *Equisetum*. A second series of experiments, conducted with greater care to exclude all waifs, gave wholly negative results. Some moulds, a coat of moss, and a single grass, *Festuca pratensis*, were the only plants within the bell-jars.

The Origin of Fairy-rings.—In "Grevillea" for March it is stated that in a recent communication to the Linnean Society Dr. J. H. Gilbert draws attention to the fact that, according to published analyses of various fungi, generally from one-fourth to one-third of their dry substance consists of nitrogenous matters. In fact, fungi would appear to be among the most highly nitrogenous of plants, and to be also very rich in potass. Yet the fungi have developed in "fairy-rings" only on the plots *poorest* in nitrogen and potass in such conditions as to be available to most other plants. They flourish strikingly on two plots only, in neither of which either nitrogen or potass is applied as manure, on which the development of grasses is extremely restricted, and their limited growth is due to a deficient available supply of nitrogen, or of potass, or of both, and, where the completion of the Leguminosæ is also weak, in the absence of a more liberal supply of potass. The questions obviously arise whether the greater prevalence of fungi under such conditions be due to the manurial conditions themselves being directly favourable for their growth, or whether other plants—especially grasses—growing so sluggishly under such conditions, the plants of the lower orders are the better able to overcome the competition and to assert themselves. On this point the further questions arise, whether the fungi prevail simply in virtue of the absence of adverse and vigorous competition, or whether to a greater or less extent as parasites, and so at the expense of the sluggish underground growth of the plants in association with them; or, lastly, have these plants the power of assimilating nitrogen in some form from the atmosphere, or in some form or condition of distribution within the soil not available (at least when in competition) to the plants growing in association with them.

Germination of the Spores of "Hemileia Vastatrix."—The germination of this curious fungus has not as yet been observed in Europe; but Dr. G. H. K. Thwaites, of Ceylon, has given the results of his experiments on germination. He says that it is not difficult to induce germination. Mature spores removed from a diseased leaf, and laid upon charcoal, kept constantly moist, commence to germinate in a few days. This process consists in the spore becoming somewhat enlarged, and its contents converted into one or more globose translucent masses. From each of these a filament is developed, which grows very rapidly, and becomes more or less branched. At the termination of some of these branches secondary spores are produced in the form of radiating necklace-shaped strings of little spherical bodies of uniform size, and this form closely resembles the fructification of an *Aspergillus.* Another observer in Ceylon (Mr. Abbay) has seen another form of secondary spores arranged in simple rows of spherical bodies—a good deal larger than those radiately arranged, but still exceedingly minute. These inconceivably numerous secondary spores may be easily transported by the slightest breath of air from place to place, and from plantation to plantation. Messrs. Berkeley and Broome have intimated that this fungus seems to hold an intermediate place between Uredines and Moulds. The germination, as well as structure of the species, is thus seen to be very unique and interesting.—"Grevillea," March 1876.

"The Garden" Botanical Newspaper now issues with each weekly number a full-page coloured plate of some interesting specimen of plant. We have seen two of the representations, and can speak in the highest terms of the efforts of the artist.

Emissive Power of Leaves.—"Silliman's American Journal" states that M. Maquenne, on comparing the quantity of water evaporated by a cultivated soil during vegetation with that furnished by the rain, finds in general an excess in favour of the former. May not this excess be caused by the dew deposition at night on the plants? When the dew is measured by a pluviometer the results are much too small. The leaves condense far more than surrounding bodies, and their temperature may fall six or eight degrees below the air, showing that their emissive power is much greater than that of the metal surfaces of the pluviometer. To determine the emissive power of leaves, a Leslie cube was employed; one of its faces was blackened, another covered with leaves, and the two surfaces turned successively to the pile. The temperature of the water did not exceed 40°, to avoid injuring the leaves. The deflections were measured by a mirror and scale, and a twentieth of a degree was easily observed. On trying several kinds of leaves it appeared that their emissive power did not differ greatly, was the same on both sides, and had an average value of 94, that of lampblack being 100. To measure the absorbent power, a thermopile was formed of a thin sheet of copper riveted to a steel spring. One face was covered with lampblack, the other with the leaf to be examined. Exposing the two surfaces in turn to the radiation of a metallic blackened box heated by steam, and waiting until the galvanometer needle came to rest, the ratio of the deviations gave the absorbent power of the leaf. From the results it appeared that the absorbent power is sensibly equal to the emissive power, and consequently the amount of dew deposited on plants should be determined by pluviometers painted black.

The Functions of the Stomata in Plants.—This subject has been recently investigated by Mr. T. Meehan, who has presented a paper on it to the Academy of Sciences of Philadelphia. From this it seems that the current ideas as to the functions of those structures are somewhat erroneous. Mr. Meehan exhibited a leaf taken from a small tree of *Acer pseudo-platanus* —the common sycamore maple—which had assumed an inverted position. The tree was three years old from seed, and all the leaves were of that character. The young leaves first appeared in their normal condition, but as the petiole lengthened, the leaf blade bent under, so that the under surface was exposed to the full sunlight, and under the petiole above. He also exhibited a young oak from an acorn sown in the spring, and which he believed to be *Quercus Catesbæi,* and in which all the leaves were vertical, and not with their surfaces on a plane with the horizon, as is the case with all other seeding oaks and American trees. He said it was possible this position of the leaves was not continued with the increased age of the tree, or it would have been observed and placed on record. Of several hundred young plants, all had the same character. The facts were simple in themselves, but had great interest to the student in vegetable physiology. The stomata were usually on the under side of the leaf, and believed to be there of a necessity. Our leading botanical text-book taught that stomata were breathing pores, and could not carry on their essential operations when exposed to direct sunlight ; and the same high authority had suggested that if leaves of this character could be inverted, and forced to remain in this condition, the plant would inevitably die. The maple did not die, but had been during all its existence as healthy as others of the same species growing near it. A large number of the proteaceous and myrtaceous plants of South Africa and Australia, and of which the now famous *Eucalyptus globulus* is a familiar example, had their leaves vertical, as in this oak. This had been accounted for by the statement that these leaves had stomata on both surfaces of the leaf, and the effort of these stomata to face the earth had of course resulted in an even balance of power, in which neither side had any advantage. The stomata on each side of the leaf had to face the horizon. Supposing this might account for the position of these oak leaves, they, as well as the maple, had been examined microscopically by Dr. Hunt, of the Academy, and found to have stomata only on one—the normal side. He thought it safe to conclude, from these facts, that the accepted theories of the relation of stomata to light required some modification.

Extraordinary Growth of Vallisneria spiralis.—Mr. A. W. Bennett, M.A.,B.Sc., asks the editor of the "Journal of Botany": Can any of the readers confirm his observation of the extraordinarily rapid growth of the flower-stalk of this plant? It was first observed in his aquarium about 10 A.M. on July 19, and measured 26 inches, this being almost certainly the growth of the previous forty-eight hours. At 10 A.M. on July 20, it measured 38 inches, or had grown 12 inches in twenty-four hours. At 4 P.M. on the same day the length was 41 inches; at 10 A.M. the next morning $42\frac{1}{4}$; and 10 A.M. on the 22nd, 43 inches, its ultimate length. From the time when first observed—the 19th—till the 31st, when he left home, the flower, a female one, remained open without any apparent change, not being fertilised during that time, as no male flower made its appearance. There was no coiling or uncoiling of the flower-stalk during the whole time; only a slight waviness. Since he published this statement, observations of a similar character have been made by Mr. W. Reeves, F.R.M.S., the Under-Secretary of the Royal Microscopical Society, and by various botanists, all showing the very rapid-growing powers of this plant. We do not know whether there is any male plant in this country.

Colossal Redwood Trees: Sequoia sempervivens.—R. E. C. S., writing in the "American Naturalist," says that, at a recent meeting of the California Academy of Sciences, Dr. A. W. Saxe made a preliminary report on a grove of colossal redwood trees that have been discovered on the course of the San Lorenzo, which takes its rise near Saratoga, in Santa Clara County, and debouches into the Bay of Monterey, at Santa Cruz. The trees are in a forest around the head-waters of the stream. One of them eclipses all that have been discovered on the Pacific Coast. Its circumference as high as a man can reach, standing and passing a tape line around, is a few inches less than 150 feet. This is beyond the measurement of any of the *Sequoias* (*gigantea*) in the Calaveras Grove. The height is estimated at 160 feet, and a part of the top lying on the ground riven off by lightning, or a tornado, is over 100 feet in lengch. The other trees in the vicinity are not as large, but all are of immense girth. Dr. Saxe promised to get information more in detail from the President of a flume (?) company in that section. This region has but recently been explored, and what other marvels of vegetation it contains remains to be seen. The stumps of redwood trees, of immense proportions, have been reported, from time to time, to the Academy, by explorers in the Mt. Diable range along the hills back of Oaklands; but now we are likely to have further discoveries of these majestic conifers in their glory, height, diameter, and foliage.

CHEMISTRY.

On Parabin, a new Carbohydrate.—Herr Reichardt has prepared from both the tissue of beets and of carrots, a new carbohydrate, which, on account of its close resemblance to Scheibler's arabinic acid, he calls *pararabyn*. The root is rasped, the pulp pressed out, treated with water and alcohol to remove everything soluble, digested for several hours with a one-per-cent. solution of hydrochloric acid, heated to boiling, and the liquid strained off. Alcohol throws down a gelatinous precipitate, which after washing with alcohol and drying, forms a friable white powder, swelling up in water and dissolving on the addition of an acid when heated. Alkalies precipitate it again, and it gives no sugar by the action of sulphuric acid. Its formula is $C_{12}H_{22}O_{11}$. It differs from arabinic acid by its neutral reaction and its chemical indifference; by its yielding no sugar; by its solubility in acids and precipitation by alkalies instead of the reverse. If, however, it is acted on for a long time by an alkali, or if it be warmed in contact with it, it is converted into arabinic acid. Quantitative experiments showed that 38·5 per cent. of the beet pulp was arabinic acid, 54·0 per cent. pararabin, and 7·5 per cent cellulose.

Ethyl Alcohol in Plants.—The occurrence of ethyl alcohol in the unfermented juices of plants is rendered probable by the fact that these juices contain not only acetic acid, its oxidation product, but also its homologues, methyl, hezyl, and octyl alcohols. Herr Gutzeit has made, in Geuther's laboratory, a somewhat extended investigation of this subject, using for the purpose *Heracleum giganteum*, *Pastinaca sativa*, and *Anthriscus cerefolium*. Six and a quarter kilograms of the fruit of the first plant being distilled with 18 kilograms of water, gave 12 kilograms of a distillate, from which the oil on the surface was separated by means of a syphon. The residue on distillation gave 12 to 15 grams of volatile liquids, which, after rectification, boiled between 72° and 77°, and consisted of methyl and ethyl alcohols, the latter constituting two-thirds. By examining the fruit at various stages of growth, the author infers that as ripening approaches, the ethyl compounds gradually diminish, being converted into more condensed bodies; while the methyl compounds remain constant. The other plants mentioned gave a similar result.

Circumstances which affect the Crystallization of Sugar.—M. Durin has a paper on this subject in the "Comptes Rendus." In the commercial analysis of sugars it is not sufficient to determine the mere quantity of pure sugar present. It is needful also to know the amount of certain foreign bodies which have the power of interfering with the crystallization of sugar in the process of refining. Salts hinder crystallization to the extent of four or five times their own weight. Glucose has also been considered as obstructive to the extent of twice its weight. The author finds that crystallizable salts do not interfere with the crystallization of the sugar, but that the formation of treacle is due to organic matters and deliquescent salts present in the juice of the cane and the beet. Nevertheless, the assumption that each part of saline matter will hinder the crystallization of four parts of sugar, though it is founded on a theoretical error, gives results satisfactory in industrial practice. Glucose, contrary to the usual opinion, does not interfere with crystallization.

A Royal Society's Medal to Mr. Crookes, F.R.S.—We are sure those of our readers who are at all familiar with chemical or physical research will be delighted to hear that a royal medal has been awarded this year to Mr. Wm. Crookes, F.R.S., for his various chemical and physical researches, more especially for his discovery of Thallium, his investigation of its compounds and determination of its atomic weight, and for his discovery of the repulsion referable to radiation. Whatever views we may hold as to Mr. Crooke's opinions on spiritualistic subjects, we can have no hesitation in expressing our extreme gratification at this mark of appreciation being shown him by the Royal Society.

Detection of Bromoform in Commercial Bromine.—It is stated that in titering, by means of potassium iodide, a solution of bromine in water, Herr Reymann observed that the result obtained was too low, and that the liquid possessed a peculiar odour recalling that of chloroform. Further investigation showed the bromine to be mixed with at least 10 per cent. of a snbstance boiling between 80° and 165°, the principal part of which consists of bromoform. It is readily detected by the influence it has in lessening the solubility of bromine in water as well as by its odour, which is most readily perceived when the bromine is agitated with a solution of potassium iodide and the whole decolorized by sodium thiosulphate.

A New Acid and Oxide of Uranium.—In a preliminary notice which Mr. T. Fairley, F.R.S.E., has sent to the "Chemical News," he explains the action of hydrogen dioxide on salts of uranium. He says it is remarkable, and takes place even in presence of much free acid. On mixing solutions of uranic nitrate and hydrogen dioxide, a yellowish white precipitate is obtained, which, when washed and dried at 100° C, retains one atom of water, and gives numbers agreeing well with the empirical formula NO_2,H_2O. Its real formula is no doubt some multiple of this formula. This oxide is, by its decomposition with alkaline hydrates, shown to be a compound of a higher oxide of uranium, NO_3, with uranic oxide, N_2O_3. The sodium, potassium, and ammonium salts of this acid have been prepared. The sodium salt is readily obtained in crystals by mixing strong solutions of uranic salt with excess of hydrogen dioxide solution (5 per cent.), and then adding strong sodium hydrate solution in quantity sufficient to dissolve the precipitate. If weaker solutions be used, the addition of a little alcohol will separate the sodium salts in crystalline plates. The full analyses of these compounds he will publish shortly. By means of hydrogen dioxide uraninm may be separated from all other metals, and in acetic solutions either uranic salt or hydrogen dioxide may be used to titrate each other using potassium ferrocyanide as indicator.

GEOLOGY.

Flint Implements from the Brixham Cavern.—We have received a series of very admirable photographs from Mr. N. Whitley, done by himself, o specimens of flint weapons which he alleges are taken from those to be seen at the Christy Museum. Of the more than thirty examples in the photographs there are hardly more than two which strike us as being of human workmanship. We have carefully examined them, and have submitted the photographs to the opinion of an eminent geologist, who thoroughly agrees with us in regarding these photographs, with one or two exceptions, as those of purely natural specimens of flint.

Oolitic Brachiopoda.—We learn from the "Geological Magazine" that Mr. J. F. Walker, M.A., F.G.S., exhibited at a late meeting of the Yorkshire Naturalists' Club the following species of Brachiopoda which occur on the Continent, but are scarcely known as British species, viz. *Terebratula* *bisuffarcinata*, Schlot., and *Rhyn. Thurmanni*, Voltz., from the lower calcareous grit of Filey, Yorkshire coast; *Waldheimia umbonella*, Lamarck, from the Kelloway rock of Scarborough; *Terebratula Eudesii*, Oppel, and *Terebratula ventricosa*, Yieten, from the inferior oolite of Cheltenham; and *Terebratula Ferryi*, Des., from the inferior oolite of Dorsetshire.

Carboniferous Conifers.—Professor J. W. Dawson writes as follows from Montreal to "Silliman's American Journal":—Little attention seems as yet to have been given to the remains of coniferous trees found in the carboniferous rocks of the United States. In Nova Scotia several species are known, and are to some extent characteristic of definite horizons. In the carboniferous sandstones of the United States such remains seem to be frequent; but I have seen no detailed account of them, and the only well characterized specimens which have come into my hands are portions of two trees from Ohio kindly sent to me by Dr. Newberry, and a very finely preserved fragment from Mazon, Grundy Co., Illinois. Both of these show the characteristic structure of *Dadoxylon* (Unger), *Araucaroxylon* of Schimper's recent work, and are closely allied to *D. materiarium*, the common species of the upper and middle coal formations of Nova Scotia. The specimen from Illinois is probably identical with that species. One of these from Ohio presents some peculiarities in the structure of the medullary rays, which may indicate a distinct species. The subject is deserving of the attention of microscopists in the coal districts, as there can be little doubt that several interesting species remain to be discovered, and other kinds of structures; as for example the curious *Dictyoxylon* of Williamson, found also in Nova Scotia, would very possibly reward patient slicing of trunks showing structure. The Devonian has also treasures of the same kind. In the United States it has already afforded *Dadoxylon Halli* from New York, and *D. Newberryi* from Ohio, besides the curious *Ormoxylon Erianium.* No doubt other species remain to be discovered, especially in the Upper and Middle Devonian. The writer of this note would willingly correspond with anyone engaged in such researches, as he has now under examination a number of specimens from different parts of British America, and would be glad to have opportunities of comparison with those of the United States.

A Query as to the Bones of the Hadrosaurus: Clavicles or Ischia?—In the Reports just issued of the Academy of Nat. Science, Philadelphia, it is said that Professor B. Waterhouse Hawkins referred to his remarks of last summer regarding the position of the so-called clavicles of *Hadrosaurus.* Having drawn a figure of the skeleton, he explained the impossibility of disposing of those bones in the position of clavicles. A comparison of the skeleton of *Hadrosaurus* with that of the ostrich was made, and the conclusion drawn that the ischiatic position assigned to the bones in question in the restoration made for the Smithsonian Institution was unwarranted. Prof. Cope stated that he had lately obtained a metatarsal bone of *Lælaps*, which confirmed the views of Professor Hawkins, as expressed in his restoration of that animal made during his engagement at the Central Park, New York.

Nomenclature of the Drift.—In reference to this subject Mr. G. H. Kinahan has written a letter to the "Geological Magazine," in which he gives an account of the Irish gravels. He says that as to those, they have never been systematically examined or classed. "We have different gravels—1st, under the 25-feet contour-line; 2nd, under the 110-feet contour-line; and 3rd, under the 350-feet contour-line—all more or less containing marine shells. Although these gravels are of distinct ages, yet the fossils collected from them have been lumped together. Then older than the Esker gravels (under the 350-feet contour-line), there are gravels at about the following respective heights—550 feet, 750 feet, and 1,200 feet, some of which contain fossils; and although these gravels must be much older than the three groups first named, yet their fossils have been all classed together. I remember hearing my brother, the late Dr. Kinahan, remark that the group of fossils from the gravels at Bohernabreena (about 200 feet) were distinct from the group found in the gravels at Howth and the coast to the northward (under 100 feet). In no place in Ireland have I seen gravels belonging to the first three groups (the third being the so-called 'middle gravels') under normal glacial drift, although I have found gravels belonging to all the others so situated."

Scotch Fossil Trees.—We learn that six trunks of large trees have been obtained at Craigleith quarry since 1826. The largest, 36 feet long and 12 to 14 feet in girth, has been taken to the British Museum, and is to be set up erect there. Another is nearly 30 feet long, and has been removed to the Botanic Gardens. The trees were conifers. The surface of each is bituminous coal, varying from one-twentieth of an inch in thickness to two inches. The trunks inside of this coaly exterior consist of carbonate of lime, carbonate of magnesia, carbonate of iron and free carbon in varying proportions. The coaly exterior is attributed, by Sir R. Christison, to bitumen passing to the surface as the destructive distillation of the wood was going on within.

MEDICAL SCIENCE.

The Brain in Man and the Lower Animals.—In the "Proceedings of the Royal Society" (No. 163) appears a paper by Prof. Marshall, F.R.S., in which the author says:—"1. I desire to communicate to the Royal Society the fact that I have, by severing the cerebral hemispheres in certain definite directions in man, and also in some of the higher vertebrata, and by then weighing the separated portions, not only arrived at some interesting and important results as to the relative size of those portions in different animals and in man, but I am enabled to state that this method, applied to the brains of individuals of different race, sex, age, education, and occupation, seems likely to furnish a means of investigating individual peculiarities in the human cerebrum. I propose shortly to communicate my results to the Society. 2. I have likewise made numerous observations on the convolutions of the human brain, with the view of explaining their symmetry in certain regions, and their asymmetry in others. In endeavouring to trace more particularly the causes of the asymmetry of the convolutions which prevails in man, I have been led to believe that some, at least, of this is due to the right-handedness of man. I find, on studying a large number of human cerebra, that there are stronger evidences of *essential* asymmetry, as distinguished from what I would term *non-essential* asymmetry, in the immediate neighbourhood of the left fissure of Rolando, and next to this part in the right parietal lobule. There are certain secondary essential asymmetrical conditions which may be pointed out, and besides this many non-essential and very variable ones. Evidence can be given in support of these propositions from the examinations of fœtal brains and the brains of idiots, the former cf which exhibit a remarkable, *early*, and special tendency to deviations in symmetry in the neighbourhood of the left fissure of Rolando.

Chloral a Substitute for Spirits in Preserving Anatomical Specimens.—The "Philadelphia American Times" contains an article by Dr. W. W. Keen upon the anatomical, pathological, and surgical uses of chloral, in which he recommends this substance very strongly for the preservation of objects of comparative anatomy and natural history. It is used by injection into the blood-vessels, or by immersion, and in his opinion it is likely to supersede many of the preparations now in use. Its special advantage is that the colour of the object is preserved perfectly, and all the parts have a natural consistency, while there is nothing either poisonous or corrosive to affect the general health of the experimenter or to injure instruments. For preserving a subject for dissection, half a pound of chloral will suffice, at a cost of a dollar or less. A solution for preserving specimens of natural history of ten or twelve grains to the ounce of water is quite sufficient, is much cheaper than alcohol, and the bottles instead of being hermetically sealed are closed by glass stoppers, or even ordinary corks. Dr. Keen has thus kept pus from various substances, and diseased growths of various kinds of other specimens, for months, and found no change whatever in their character. Chloral is extremely antagonistic to fungi and infusoria, a very weak solution of it killing them instantly. The deodorizing as well as the antiseptic properties of chloral are equal, in Dr. Keen's opinion, to those of any substance now known.

The Alkaloids contained in the Aconites.—Dr. Wright, of St. Mary's Hospital, has been investigating this important subject. In a paper read before the Chemical Society he says that the material for this investigation was furnished by Mr. T. B. Groves, who prepared it in the manner described in the "Year-Book of Pharmacy," 1873 and 1874. The authors describe several bases which they have isolated from this mixture, one of which, having the composition $C_{31}H_{45}NO_{10}$, appears to be comparatively inert, whilst another, of the composition $C_{33}H_{45}NO_4$, has a powerful physiological action. A pseudaconitine, $C_{36}H_{49}NO_{11}$, which forms indistinctly crystalline masses, was also obtained. The authors consider that either the roots of the aconites contain several alkaloids capable of crystallising or of giving crystalline salts, or else that the alkaloid originally present is readily alterable and gives rise to numerous altered products during the processes of extraction and purification.

An American View of British Rivers.—The New York "Graphic" tells us some very extraordinary facts!! It says that "the pollution of rivers is a topic that taxes the attention of British sanitarians to the utmost. The extent of the difficulty is certainly alarming. Nor is it caused solely by the sewage of cities and towns, bad as that is. The rivers are lined with manufactories of all sorts, chemical works, machine shops, and dye-houses, all of which pour their poisonous refuse into the rivers. And so great is the pollution of the water in some instances that when a light was applied to some of it dipped from the river at Bradford it actually burned. A man who accidentally tumbled into a river and swallowed a mouthful of water died of the effects. The effluvia that rises from the Clyde produces sickness in summer time, and the Mersey emits an unbearable stench. The water of the Bourne is yellow as ochre and thick as glue, and the horse that drinks it dies. And all the rivers are more or less affected in the same way, and the fish that survive in some of the streams are so unwholesome that they are unfit for food, if not dangerous."

Respiration in Insects.—In a paper recently published (in the "Central-Blatt für Agri. Chem." Heft 6) Herr O. Bütschli records some experiments which he conducted on the common cockroach (*Blatta orientalis*). From these we learn that the liberation of carbonic acid was found to be directly proportional to the temperature.

Variations in the Strength of a Muscle.—Professor F. E. Nipher publishes a valuable paper on this subject in "Silliman's American Journal." The conclusions drawn by the author are :—1. After the relation of the strength of a muscle to the dynamical work of exhaustion has been determined, the strength at any time will be most accurately determined by measuring the dynamical work of exhaustion. On days when from any cause the muscles are temporarily weak, the strength as determined by the dynamometer, and the work of exhaustion with very heavy weights, is less. In exhaustion with lighter weights, however (5. kgr.), the exercise of the first part of the experiment appears to invigorate the muscle, and the influence of temporary weakness, due to errors in diet, or lack of exercise, or the oppressive atmosphere of the room, is eliminated. 2. The coefficient of muscular power per square centimeter of section of the muscle, is a quantity which varies greatly with different muscles, and with the same muscle at different times ; or, in other words, the work which a muscle can perform depends not only upon its size but also upon its quality.

The Body rendered Luminous by Phosphuretted Hydrogen.—The editors of "Silliman's American Journal" state that Dr. Geo. Maclean, of Princeton, in a private communication to them, says :—"Several years ago, after spending a portion of the day in experimenting with this gas (PH_3), prepared from phosphorus and solution of potash, on retiring to bed I found my body quite luminous, from a glow like that of phosphorus exposure to the air. Either some of the gas escaping combustion, or the product of its burning, must have been absorbed into the system, and the phosphorus afterwards separated at the surface have there undergone eremecausis. I was conscious of no feeling that could be attributed to it, nor was my health apparently in any way affected by it." Probably a repetition of the trial would reproduce the results observed by Dr. Maclean, which have apparently escaped notice heretofore because unsought.

Unipolar Electric Excitement of the Nerves.—The "Chemical News," giving an account of a paper lately read before the French Academy, says that M. A. Chaveau states that for every subject whose nerves are in a perfect physiological state there is an electric value — commonly very weak, sometimes moderate, but rarely very high—which gives to the two poles the same degree of activity in the case of unipolar excitement of the motor nervous bundles. The contractions produced by positive and negative excitement with this typical intensity of the current are equal, both in magnitude and duration. Below this intensity equal currents produce unequal effects with the two poles, the activity of the negative pole being more considerable. When tetanisation is produced by feeble currents, it is never with the positive pole upon the nerve. Above the typical value of the intensity of the current the inequality appears in the opposite direction.

METALLURGY.

Samarskite in America.—At a recent meeting of the Academy of Philadelphia, Mr. Joseph Willcox called attention to a mineral specimen (samarskite) presented by him, which was found at a locality discovered by him recently among the mountains in Mitchell Co., North Carolina. Excepting in North Carolina, this rare mineral is only found in the Ural Mountains in Asia. According to Dana's "Mineralogy," the largest specimens found at the latter locality are only as large as hazel-nuts; but Mr. Willcox said he obtained a specimen in North Carolina that weighs more than twenty pounds. It was associated with decomposed feldspar.

The Association of the Native Platinum of the Urals.—A writer in the "Geological Magazine," whom we take to be Professor Morris, says that M. Daubrée, in an interesting paper before the Academy of Sciences, has shown that native platinum, although obtained abundantly in the alluvial deposits of certain regions of the Ural, has been found in a Peridote (Olivine) rock, which is more or less altered into serpentine, and accompanied with diallage (a ferruginous sahlite, according to M. Des Cloiseaux), and also with chromite, which occurs abundantly, not only in separate grains, but also encrusting the grains of platinum. The platinum, which is here associated with chromate of iron, appears to be distinguished from the platinum of other deposits by the large proportion of metallic iron with which it is alloyed. It appears that platinum very rich in iron, and endowed with magnetic polarity, has not been found—at least, at present—save in company with chromate of iron.

MICROSCOPY.

The New Platyscopic Pocket Lens promises to have a very great success, for it certainly completely distances the Stanhope or Codrington in largeness and flatness of field, if not in magnifying power. It is certainly the best pocket lens that we have ever seen, and we have very carefully examined and tested it before expressing this opinion. It is a triple achromatic combination, in which the chromatic and spherical aberrations are corrected by the central lens of dense flint. This lens is nearly three times as thick as the crown-glass lenses. The interior curves are almost hemispheres. The final correction of spherical aberration is made by altering the thickness of the dense flint glass lens. The three lenses are united by a transparent cement which has a refractive index corresponding very nearly with the glass. This prevents light being lost by reflexion from the surface of the deep curves. The new lens is manufactured by the inventor, Mr. J. Browning, the optician, of 111 Minories, E.C.

Diatomaceæ to be Obtained from Coal.—The "Monthly Microscopical Journal" says that the following method of obtaining the diatomaceæ from coal for the microscope is described by Count Castracane :—" The course to pursue is decided by the flinty nature of the diatom valves, and in order to separate them from the mixture of calcareous or organic matter with which they are found united, it is usual to put the whole into a glass test-tube with hydrochloric acid, adding chlorate of potash from time to time, keeping all slowly dissolving by heat, in order to isolate the silex, destroying the remainder. But in unburnt coal it is too difficult to dislodge the carbon, and the acids have little effect upon it. I must, however, refer to the calyination I effected by grinding up the substance, and then, collecting it in a china vessel, placed upon a stove in a glass tube, subjecting the whole to the action of the heat, while, at the same time, a slight current of oxygen crossing the tube combined with the carbon in creating acid. Experience has taught me, however, the necessity of conducting this operation at a lower temperature, in order to prevent the alkaline or earthy bases and metallic oxides, which may be amongst the ashes, from forming vitreous silicates by melting and mixing with the valves of the Diatomaceæ. It is also well to leave the glass tube, in which the fusing is going on, uncovered, in order to watch its progress. The small residue obtained through this process is to be put into a clean test-tube, adding nitric acid and hydrochloric acid and caustic potash, assisted by the heat of a lamp, to eliminate any alkaline or earthy base and every trace of metallic oxides. The last operation over, it only remains to wash rapidly with distilled water the very light dust which is left behind, letting it stand for some hours to settle, in order to be sure of not losing the smallest particle of it in pouring off the water. Those who follow this method exactly cannot fail to succeed. The object may then be mounted with Canada balsam, or in any other suitable medium ; and steadily and closely watching it, under the microscope, they will not be long before they see some valves of diatoms, entire or broken."

The Leaf Glands of Saxifraga tridactylites.—The "Monthly Microscopical Journal" of Jan. says that an addition to our list of carnivorous plants is suggested by Mr. J. C. Druce, in a letter to the "Pharmaceutical Journal," in a little early spring flower found chiefly on the tops of walls, *Saxifraga tridactylites*, a plant not very distantly allied to the Droseras. Mr. Druce states that when examined under the microscope the leaves are seen to be covered with glands of a similar character, which exude a viscid secretion, in which he found a midge was entrapped and held fast when placed on the leaf. On examining a number of leaves, he found in all of them the débris of insects which had apparently perished in this manner.

Mode of looking at a Watch through a Beetle's Eye.—Dr. Whittell, of Adelaide, Australia, has a paper on this subject in the "Monthly Microscopical Journal," March, in which he gives the following instructions as to the mode of mounting the specimen :—Take a watch with a white face, take out the front glass, and remove the hour and minute hands. Paste over the face of the watch a piece of dead-black paper with a round window cut in it, so as to leave nothing exposed but the small circle in which the seconds hand rotates. Place the watch on the front of the mirror of the microscope, and condense the light of a strong flame on the small white circle that has been left exposed. Reflect this light through the beetle's eye, previously placed on the stage, just in the same manner as if the ordinary mirror were being employed. Bring the eye into focus, and then gradually draw back the objective by means of the fine adjustment until the images of the watch hand appear. At first these will probably be dim, but by varying the inclination of the watch and careful adjustment of the light the observer will at length obtain a bright and distinct image through each lens of the eye. The nearer the watch can be brought to the stage without cutting off light from the condenser, the larger will be the image. Any power may be used from $\frac{1}{8}$ to $\frac{1}{3}$ inch, but I prefer a $\frac{4}{10}$th, with a No. 2 eye-piece. Under this power the images are sufficiently enlarged, and a good number of them are included in the field. The eye may be mounted in balsam, but I think I have obtained better results from one specially prepared and mounted in glycerine.

Microscopical Papers of the Quarter.—The following is a list of the contents of the "Monthly Microscopical Journal" for Jan., Feb., and March 1876:—

The Absorptive Glands of Carnivorous Plants. By Alfred W. Bennett, M.A., B.Sc., F.L.S., Lecturer on Botany at St. Thomas's Hospital.—Reproduction in the Mushroom Tribe. By Worthington G. Smith, F.L.S.—Avoiding the Use of the Heliostat in Micro-photography. By G. M. Giles, M.M.M.S.—Improved Method of Applying the Micro-spectroscopic Test for Blood-stains. By Jos. G. Richardson, M.D., Attending Physician to the Presbyterian Hospital; Microscropist to the Pennsylvania Hospital.—Remarks on the Foraminifera, with especial reference to their Variability of Form, illustrated by the Cristellarians. By Professor T. Rupert Jones, F.R.S., F.G.S.—The President's Address. By H. C. Sorby, F.R.S., F.L.S., F.G.S., F.Z.S., &c.—Further Notes on Frustulia Saxonica. By W. J. Hickie, M.A., St. John's College, Cambridge.—On the Characters of Spherical and Chromatic Aberration arising from Excentrical Refraction, and their relations to Chromatic Dispersion. By Dr. Royston-Pigott, M.A., F.R.S., F.C.P.S.—On Staining and Mounting Wood Sections. By M. H. Stiles.—On a Mode of Viewing the Seconds Hand of a Watch through a Beetle's Eye. By Dr. Whittell.

PHYSICS.

Utilising the Solar Rays.—A method for this purpose has been described before the French Academy by M. A. Mouchot. The author's apparatus is composed of three distinct pieces—a metallic mirror with a linear focus; a blackened boiler, the axis of which coincides with this focus; a glass enclosure, which allows the solar rays to reach the boiler, but opposes their exit as soon as they have been transformed into heat-rays. The yield of a large solar heat-generator is greater than that of a small one. The whole apparatus is arranged so as to turn 15° hourly around an axle parallel to the axis of the earth, and to incline gradually to this axle, according to the sun's declination. An apparatus of this kind, erected at Tours, gave the following results:—In ordinary fine weather, 20 litres of water at 20°, introduced into the boiler at 8.30 a.m., produced in forty minutes steam at a pressure of 2 atmospheres, or a heat of 121° cent. This steam rapidly rose to the pressure of 5 atmospheres, a limit which it was not safe to exceed. About mid-day, with 15 litres of water in the boiler, the steam was raised in fifteen minutes from 100° degrees to 153°, a pressure of five atmospheres. Hence the author concludes that in hot and sunny climates the sun's rays may be successfully utilised as a source of mechanical power.

A Monster Battery.—A chloride of silver battery of 3,240 elements has been described by Mr. Warren De la Rue and Mr. A. W. Müller. The description has appeared in French, but it is thus translated in the "Chemical News." It is composed on the one part of 1,080 elements, each consisting of a tube of glass 15·23 c. in length, and of 2,160 elements formed of glass tubes of 12·75 c. in length only. All the tubes are 1·9 c. in diameter, and are closed with stoppers of vulcanised india-rubber, perforated with a hole near the edge to permit the introduction of a rod of amalgamated zinc, 0·48 c. in diameter, and 10·43 c. in length for the first 1,080 elements, and 7·93 c. for the remainder. At the bottom of each tube powdered chloride of silver is placed, 14·59 grms. in weight, compressed strongly with wooden rods, a flattened silver wire having been first introduced to the bottom of the tube. The silver wires are covered in their upper part, above the chloride of silver, and up to the point where they emerge from the vulcanised stopper, with leaf gutta-percha to isolate them and preserve them from the action of the sulphur in the stoppers. The electromotor force of this battery is to that of a Daniell's battery as 1·03 to 1.

A Novel and Useful Form of Bunsen Burner has been described in the "Chemical News," by President Henry Morton, Ph.D., of New Jersey, in the following terms:—"The retreat of a burner will evidently occur whenever any part of the ascending column of mixed gas and air is moving at the orifice with a velocity less than that at which the same will burn downwards. Now, in an ordinary burner, with its main tube of regular cylindrical bore, it is evident that the friction of the surface of the ascending column of mixed gases will cause that portion to move at a less velocity than the central part, and that currents of the nature of eddies will be developed. It will thus happen that while the central portion of the ascending column of gaseous mixture issues at a velocity much greater than that at which the material can burn downwards, and thus is quite free from any danger of retreating, the marginal portions of the column or jet of gas will be escaping at a rate so much less that the velocity of their combustion downwards will exceed that of their upward motion, and retreat of the flame will ensue. It is well known that to secure a jet of water, or of any other fluid whose particles shall move with equal velocities in all parts, and thus avoid currents and eddies, it is only necessary to make the orifice of efflux an aperture in a thin wall. Following out the idea above indicated, I made a burner of a bore rather large compared with its height, and then drew in its upper edge into the form of an open-ended thimble, so contracting the orifice of escape to about two-thirds the area of the tube, and rendering this orifice practically an opening in a thin horizontal wall or plate. The results of this modification far surpassed my anticipations. A burner thus constructed gives a perfectly nonluminous flame with gas pressures varying between 1·5 and 0·1 inch of water, and with the lowest of these pressures cannot be made to retreat by the most violent handling in the way of sudden movement or waving about in the air, even when this violence is carried to the extent of extinguishing the flame altogether. Under like conditions of pressure, a burner of the ordinary construction is made to retreat by a slight draught of air, or a very moderate amount of motion."

The last Form of French Balloon is thus described in a recent paper published in the "Chemical News," on the subject of the "development of the chemical arts during the last ten years," by Professor A. W. Hofmann. Speaking of aeronautics, he says the last impulse was given, not by festivity, but by the terrors of war and the siege of Paris. The Académie des Sciences commissioned one of its members, Dupuy de Lôme, to make experiments on steering balloons, and the Government furnished the requisite means. Dupuy gave his balloon the fish shape, and, in order to render its shape stable in the wind, he fitted it with an internal secondary balloon (*ballonet*), containing more or less air, and equal in bulk to one-tenth part of the main balloon. The air could be let out of this inner balloon by valves, or driven in again by means of a bellows in the car, according to a plan which Meusnier had devised as early as 1783 to supersede the use of ballast. Dupuy's balloon was further distinguished by a very firm method of suspending the car, and by the use of a varnish impermeable to gases, and made of gelatin and tannin dissolved in pyroligneous acid. The propelling screw was not turned by a steam-engine, but by eight men in the car. The balloon, containing 3,450 cubic metres, was filled with hydrogen obtained from iron and sulphuric acid, and went up at Vincennes on February 1st, 1872, carrying fourteen persons. After a flight of two hours, it was let down at Noyon, a distance of 106 kilometres. By means of an anemometer, Dupuy was able to determine the independent speed of the balloon at 2·82 metres per second, whilst that of the wind was 16 to 17 metres, and the course of the balloon made an angle of 12° with the direction of the wind. The problem of steering had, therefore, been solved, though only to a very slight degree—sufficient for a calm, but insufficient for overcoming even moderate winds. The speed attained was slight. Both conditions of success depend on the employment of stronger sources of mechanical power, and this, again, requires an increase of its power of ascent, *i.e.* of its relative levity with an enlarged volume.

Gyratory Movements of Salts in Water.—M. M. H. Lescœur says ("Bull. de la Chimique de Paris"), that substances possessing the *epipolic* force may be arranged in two classes:—1. *Bodies Insoluble in Water.*—When once they are spread out all motion is arrested. and the movement of every other body is suspended (fixed oils, fatty bodies, &c.). 2. *Soluble or Volatile Bodies.*—The superficial layer produced instantaneously dissolves, or is volatilised. The movement is continuous. The saturation of the liquid and ambient air causes all activity to cease (camphor, acetic acid, essential oils, &c.). As to the first cause of the motion, it is found in the reciprocal action of two fluid surfaces. It may be a phenomenon of capillarity, or of the superficial tension of the liquids.

Electro-Conductibility of Pyrites.—M. H. Duffet, in the "Comptes Rendus," contends that the conductibility of iron pyrites is a true metallic conductibility, very variable according to the physical structure of the specimen, but which, in a given crystal, is affected neither by the direction, nor the intensity, nor the duration of the current.

Common and Magnetic Iron in the Atmosphere.—It is alleged by M. G. Tissandier, who, as our readers are aware, is an eminent balloonist, that in specimens of atmospheric dust examined by him he has discovered minute particles of magnetic iron ore, which he believes to be of cosmic origin.

Influence of Forests on the Quantity of Rain.—The "Chemical News" says that the absolute quantity of moisture in the atmosphere appears to differ little at any season of the year in open grounds, and in forests. The relative moisture is, however, greater in forests, the difference being greatest in summer, and increasing with the elevation of the place above the sea-level. According to Ebermayer's view, forests increase the amount of rain only by their action upon the relative moisture of the atmosphere.

Spectroscopic Reactions of Hæmoglobin and its Derivatives.—M. C. Husson gives the following observations, according to a notice in the "Chemical News."—Hæmoglobin, on absorbing iodine, splits up into hæmatin and globulin. This fact is proved by the spectral analysis, which gives between C and D the absorption ray of hæmatin, which does not seem to be affected by the iodine. Chautard has already shown that this element has no influence upon the rays of chlorophyll. The microscope also indicates the splitting up of hæmoglobin. With bromide of potassium we obtain crystals of hydrobromate of hæmatin, of a rosy tint. On treating blood with borax and glacial acetic acid, we obtain all the crystals described by MM. Robin and Verdeil in their "Traité de Chimie Anatomique" under the name of hematoidin. Glacial acetic acid alone gives, without the aid of any other reagent, fine crystals of acetate of hemine.

ZOOLOGY AND COMPARATIVE ANATOMY.

Effect of Strychnia on Medusæ.—An interesting paper, describing the development of Medusæ, and the effect on them of certain poisons, is given by Mr. G. J. Romanes, F.L.S., in the Proceedings of the Royal Society (No. 165). He states that strychnia exerts a very marked influence upon Medusæ. "Of the species I have met with *Cyanæa capillata* is the most suitable for showing the effects of this poison, from the fact that, in water kept at a constant temperature, the normal pulsations of this animal are as regular as are those of a heart. Shortly after a solution of strychnia has been added to the water in which a specimen of *C. capillata* is contained, unmistakable signs of irregularity in the pulsation of the animal supervene.

This irregularity then increases more and more, until at last it grows into well-marked convulsions. The convulsions manifest themselves in the form of extreme deviations from the rhythmical character of the normal contractions, amounting, in fact, to nothing less than tonic spasms. It is further of importance to remark that the convulsions are very plainly of a paroxysmal nature—prolonged periods of uninterrupted convulsions being every now and then relieved by shorter periods of repose, during which the Medusa remains perfectly motionless in a fully expanded form. *C. capillata* will live for many hours when under the influence of strychnia, but eventually death supervenes. The animal dies in full diastole."

The Development of the Bird is thus summed up by Mr. A. S. Packard, Junr., in the "American Naturalist":—1. Partial segmentation of the yolk. 2. The embryo develops much as in the bony fishes until the embryonal membranes appear. 3. Formation of an amnion. 4. After the alimentary canal is sketched out, the allantois buds out from it. 5. The avian features appear from the sixth to the tenth day. 6. The embryo leaves the egg in the form of the adult, and like the reptile, is at once active, feeding itself.

How are the Chameleon's Changes of Colour produced?—A very interesting paper on this subject is that of the Rev. S. Lockwood, in the "American Naturalist" for January 1876. He gives the following admirable illustration of the changes of colour. He says:—Supposing through a sheet of block tin many thousands of little pipes were made just to enter. Let them, if you will, be regarded as infinitely small. Call this series A. Now suppose another series in all respects similar and fixed in like manner. Call this series B. It must be understood that the pipes of one series alternate with those of the other series, so that it shall be first a pipe of A, then a pipe of B, and so on in regular order for both series. Suppose again that the A pipes contain green pigment, and the B pipes contain yellow. We will further imagine that each pipe series has a series of muscles which can act upon them. Now laid over the mouths of all these pigment tubes let us suppose a translucent film. Our perforated block tin and its translucent spread, with the mouths of the colour-tubes opening between them, shall represent the *rete mucosum*, or coloured layer of the skin. Suppose now the appropriate muscles squeeze the lower ends of the A series of pigment-tubes, the pigment at once comes up against the almost transparent skin, the colour of which is now blue. Let the muscles relax and the pigment descends into the tubes again. Let the same process occur with the B series of tubes, and the result will be that the skin shows a yellow colour. Not waiting for the yellow pigment to return into the tubes, let the A series be again squeezed, and up comes the blue pigment against the translucent spread. Now everybody knows that a green colour is easily made by a mixture of yellow and blue. Suppose the little spots where the blue touches under the translucent film to be so small as to be called molecules, and suppose the same of the spots where the yellow pigment touches, and you have all the conditions necessary for begetting green. It is also easily imagined how by regulating the amount of muscular pressure the proportions of the separate pigments is regulated, and so the most delicate tints are produced.

Digestion in Insects.—A contemporary states that M. Plateau finds that when the salivary glands of insects are not diverted from their primitive function to become silk or poison glands, they secrete a neutral or alkaline liquid, possessing, at least as regards one pair, the property characteristic of the saliva of vertebrate animals of rapidly transforming starch matters into soluble and assimilable glycose. The change is effected in a posterior dilation of the œsophagus. At this place results in the carnivorous insects a transformation of albuminous matters into soluble substances like peptone, and in vegetable-feeding species an abundant production of sugar out of the starchy matter eaten. When digestion has taken place in the œsophagus, it is submitted to an energetic pressure in the gizzard or proventriculus, which is armed with teeth. It thus seems that this is not an apparatus for crushing the food, but for expressing the liquid from the food triturated by the jaws. In the stomach, or middle intestine, as Plateau calls it, the food is again submitted to the action of an alkaline or neutral liquid secreted by local glands, present in the Orthoptera, or by a great number of small glandular cæca as in many beetles, or by a simple lining of epithelial cells. This fluid has no analogy with the gastric fluid of vertebrate animals. Its function differs according to the group to which the insect belongs. In the carnivorous beetles it makes an emulsion of the greasy matters; in the Hydrophilid beetles it continues the conversion of starch into glycose, begun in the œsophagus. In the caterpillars of the butterflies and moths it determines a production of glycose and makes an emulsion of greasy matters, and in the grasshoppers no sugar is formed in the intestine, as this material is produced and absorbed in the œsophagus.

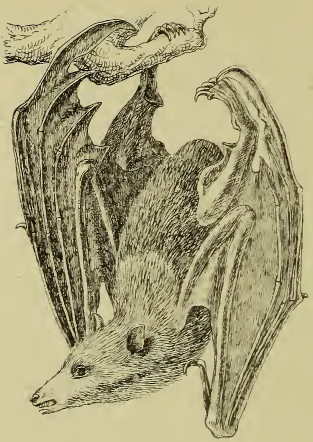
The Corals of the Galapagos Islands have been investigated by Count Pourtales, who has published the following account ("Silliman's American Journal"):—The Galapagos Islands are, as is well known, an important point in the geographical distribution of corals, being almost exactly on the boundary of the coral-producing part of the Pacific Ocean, and that portion which is destitute of them on account of the low temperature of the water. All the writers on the subject have placed this group of islands in this latter portion. During the visit of the United States Coast Survey steamer *Hassler*, a number of specimens of corals, of which the following is the list, were picked up on the beaches of several of the islands:—*Ulangia Bradleyi*, Verrill, Indefatigable Island; *Pavonia gigantea*, Verrill, James Island; *Pavonia clivosa*, Verrill, Indefatigable Island; *Pavonia*, sp., James Island; *Astropsammia Pedersenii*, Verril; *Pocillipora capitata*, Verrill, Jervis and Charles Islands; *Porites*, sp. The undetermined *Pavonia* is a massive species with larger calicles than those of the two other ones, and more porous and lighter. The specimen is too much rolled for nearer determination. The *Porites* is massive also, and in the same condition. The species are all, or nearly all, identical with those found at Panama. They are mostly reef-builders, but here live probably isolated and at a certain depth, having never been observed *in situ*. In individual growth they are fully equal to those from more favoured localites, the rolled pieces of *Pavonia* measuring six or seven inches in diameter, thus indicating masses of considerable size originally. They are not confined to the northernmost islands of the group, where we should more naturally look for them, from the greater proximity to the warm current; but, as the list shows, a *Pocillipora* was found at Charles Island, one of the southernmost. The probability of fragments drifting from one island to the other is very small, owing to the considerable depth of water between them.

The Development of the Ascidian like that of a Vertebrate Animal.—One of the best accounts of the development of ascidia that we have seen in English is that in the "American Naturalist," by Mr. A. S. Packard, Junr. Mr. Packard says—after going into the subject at length, and with the aid of various woodcuts—it will be seen that some highly important features, recalling vertebrate characteristics, have occurred at different periods in the life of the embryo ascidian. Kowalewsky remarks that "the first indication of the germ, the direct passage of the segmentation cells into the cells of the embryo, the formation of the segmentation cavity, the conversion of this cavity into the body cavity, and the formation of the digestive cavity through invagination—these are all occurrences which are common to many animals, and have been observed in Amphioxus, Sagitta, Phoronis, Echinus, &c. The first point of difference from other animals in the development of all vertebrates is seen in the formation of the dorsal ridges and their closing to form a nerve-canal. This mode of formation of the nervous system is characteristic of the vertebrates alone, except the ascidians. Another primary character allying the ascidians to the vertebrates, is the presence of a *chorda dorsalis*, first seen in the adult Appendicularia by J. Müller. This organ is regarded by Kowalewsky to be functionally, as well as genetically, identical with that of Amphioxus. This was a startling conclusion, and stimulated Professor Kupffer of Kiel to study the embryology of the ascidians anew. He did so, and the results this careful observer obtained led him to fully endorse the conclusions reached by Kowalewsky, particularly those regarding the unexpected relations of the ascidians to the vertebrates, and it would appear from the facts set forth by these eminent observers, as well as Metschnikoff, Ganin, Ussow and others, that the vertebrates have probably descended from some type of worm resembling larval ascidians more perhaps than any other vermin type, though it is to be remembered that certain tailed larval Distomæ appear to possess an organ resembling a *chorda dorsalis*, and farther investigation on other types of worms may lead to discoveries throwing more light on this intricate subject of the ancestry of the vertebrates. At any rate, it is among the lower worms, if anywhere, that we are to look for the ancestors of the vertebrates, as the Cœlenterates, Echinoderms, the Mollusks, Crustacea and Insects, are too circumscribed and specialized groups to afford any but characters of analogy rather than affinity.

The Early Stages of the American Lobster.—Mr. S. I. Smith, whose paper on this subject appeared in abstract in a number of "Silliman's American Journal" (1872), has now published the essay in full, accompanied by a series of plates, and has been good enough to forward us a copy. His paper is of considerable length, yet, from the circumstance that he was unable to study the different forms in their living state, none of the internal anatomy is given. Nevertheless, the memoir is of value and will repay perusal, although it extends over too much space to render an abstract possible. We may observe that the terms employed are those which are used by Milne Edwards, Latin being substituted for French.

Plate CXXXVI

1



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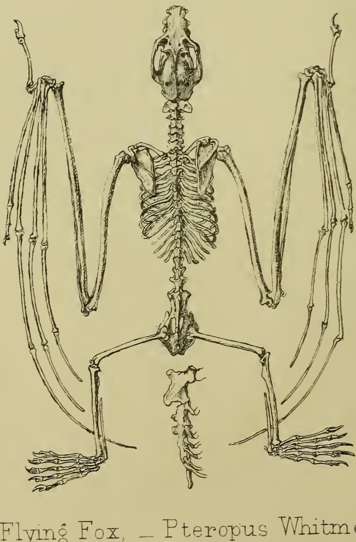

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1. Flying Fox, — Pteropus Whitmeei.
2. Skeleton of Flying Fox. — 3. Side view of Sternum.

WHAT ARE BATS?

By St. GEORGE MIVART, Ph.D., F.R.S.

[PLATE CXXXVI.]

THE group of animals called "Bats" is one full of interest to those specially occupied with the study of animal structure—the anatomist, the physiologist, and the philosophical zoologist. At the same time it must be confessed that bats are far from exciting that general interest which in fact they merit. This disregard, however, is very natural. The small size of the bats inhabiting this and other parts of the temperate regions of the globe conspires with their nocturnal habits to remove them from general observation, while the great similarity one to another of their different species is an obstacle to their popularity even amongst zoologists—since it makes their discrimination and classification a matter of difficulty.

Yet bats are, as I hope we shall see, really *very* interesting animals. The bat exhibits to us the body of a beast, specially modified to live the life of a bird, and at the same time serves to give us a fair conception of certain ancient reptilian forms, the remains of which are found deeply buried in deposits made untold ages ago—in the secondary rocks.

But what is a bat? Probably not one of my readers would be likely, if called upon to answer, to fall into the old error of considering it a kind of bird!

All who have ever examined a bat closely, and observed its fur, ears, and teeth, must, I think, have recognised it as a kind of beast. Its real affinities, however, serve excellently well to demonstrate how little mere external aspect can be trusted as a guide to fundamental relationship. The *bat* is essentially an animal of the air—all its structure is modified for flight, and it rarely *descends* to the surface of the ground. The *mole*, on the contrary, is essentially an animal of the earth—all its structure is modified for burrowing, and it rarely *ascends* to the surface of the ground. The contrast could hardly be more complete, and yet the bat and the mole are cousins—the mole, the hedgehog, and the shrew mouse belonging to a group of beasts with which the bats show no inconsiderable affinity.

I have spoken of the opinion that the bat is a kind of bird. This view seems to have been entertained by the Jews, and the "bird of darkness" is placed in Deut. xiv., v. 18 amongst the unclean ones forbidden as food :—

"And the stork and the heron after her kind, and the lapwing and the bat."

Aristotle, though he placed the bats amongst flying animals, and therefore amongst birds, distinctly recognised the differences in their organisation, and the same thing may be affirmed of Pliny. But in spite of this, and although Albertus Magnus, in the Middle Ages, was fully acquainted with the true nature of bats, as beasts, as well as with their winter torpidity, we find later on a retrogression of opinion.

Thus Belon, in 1557, in his *Histoire de la Nature des Oyseaux*, includes bats with his birds. At the same time, he was not unacquainted with the mode of their reproduction, as the following verse proves :—

> La souris chauve est un oiseau de nuict
> Qui point no pond ; ains ses petits enfante,
> Lesquels du laict de ses tetins sustante,
> En petit corps grande vertu reluit.

Yet later—by nearly a century—in 1645, Aldrovandus decided that bats were rather birds than beasts, and this in spite of his careful study of them, as proved by his beginning to distinguish their different kinds one from another.

Some twenty-five years later, Ray gave them their true position amongst quadrupeds—a position which they have ever since retained.

The Teutonic mind seems early to have appreciated the true nature of bats, as we may judge from the German name, *Fledermaus*, and the old English term, *flittermouse*.

Let us look a little closely at our subject of to-day—the bat.

In the first place, there is a little rounded body, covered with soft fur, which is indeed, what Shakspeare calls it, "wool," when giving the ingredients of the cauldron of Macbeth's witches.

There is a small head, little eyes, large ears, a tail, and two pairs of limbs of very unequal size. The hind pair (the legs) are of moderate length and singularly disposed, so that the knees are turned almost backwards, like our elbows.

Each leg terminates in a foot, furnished with five toes, each with a long, curved claw, all of about the same length. These toes are not webbed, like those of a duck, but are free.

The other pair of limbs (the arms and hands) are of exceeding length. Both the arm and forearm are long—especially the latter—but it is the fingers which are so wonderfully drawn out, and they *are* webbed, like the toes of a water-fowl. Moreover, the web not only connects these long fingers together, but also connects them with the sides of the body and with the legs (as far as the ankle); and does not stop even here, but continues on to the tail, thus connecting it with the two legs.

This large web or membranous expansion has two names. The part belonging to the hand and joining the sides of the body (which is supported by the fingers as an umbrella by its rods) is termed the *alar membrane.* The part connecting the legs with the tail is called the *interfemoral membrane.*

Looking more closely, however, we find that though the four fingers of each hand are thus bound together, the thumb is free, standing out at a wide angle, and furnished with a very long and strong, hooked claw. Of the four fingers, it is only the first which is clawed.

The uses made by the bat of its singularly-formed limbs are, of course, in exact correspondence with their structure. The fore-limbs are true organs of flight; the hind-limbs and tail have a rudder-like action. Besides flight (their predominant mode of motion), bats can crawl upon the surface of the earth with an awkward, shuffling gait. When so crawling, the wings are closed (the long fingers then lying side by side) and the animal rests on its wrists and hind-feet, the body being dragged forward by the help of the strong, hooked thumb nails, which also help it to climb with ease up any rough surface, even though perpendicular.

When at rest, bats usually hang suspended, head downwards, by the claws of their feet, though occasionally they turn round and hang from the claws of their thumbs.

Most nocturnal beasts have large eyes, but most bats have very small ones.

This is perhaps due to the fact that bats in their flight are guided by an extraordinarily delicate sense of touch—so delicate as to seem almost like a sixth sense.

HEAD OF LARGE-EARED BAT.
t, Tragus.

The external ear of most bats appears at first to be double—a very small one seeming to stand up inside the larger one. This appearance, however, is due merely to the very large development of a little piece which in ourselves projects backwards as a small rounded process guarding externally the opening of our ear, and called the *tragus.*

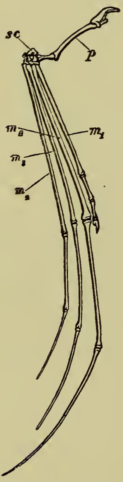
The food of our English bats consists of insects, and their teeth bristle with sharp points, well suited to pierce the chitinous cases by which the bodies of insects are protected.

The stomach (like that of most beasts which live upon a purely animal diet) is a simple, short and rounded bag.

The female is provided with a pair of milk glands, situated on the breast—as in the apes and in man.

The skeleton of the bat, when compared with those of some other animals, affords an excellent example of how fundamental uniformity of structure may underlie forms which are strikingly different—in accordance with diverging habits of life.

I have already called attention to the divergent aspects of the aerial bat and the subterranean mole. Yet the bones of the flying organ of the bat closely resemble that of the burrowing organ of the mole, save as regards the relative shapes and dimensions of the component bones. But while in the bat these bones are drawn out into excessive length and tenuity, in the mole they exhibit the maximum of concentration and robustness. Now both these conditions are but diverging manifestations of the human structure, and the same indeed may be said of such extreme modifications as the fore-leg of the horse or the paddle of the whale.



sc, wrist-bones; *p*, bones of thumb; m_{1-4}, bones of middle part of hands.

But the bat and the mole present us with a special point of similarity in their skeleton not found in the other animals named, including ourselves.

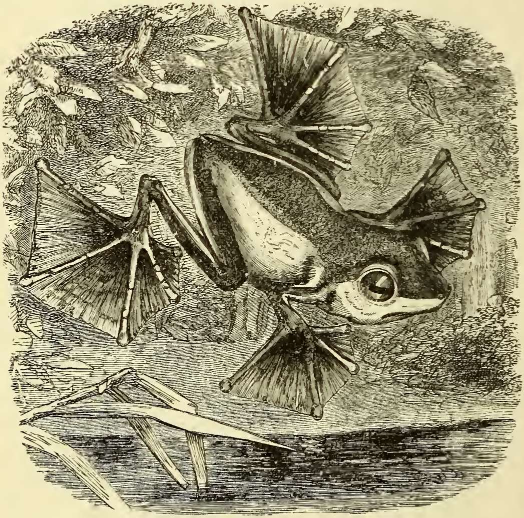
It is that the breast-bone in both the bat and mole develops a median ridge or keel. This keel serves to afford additional surface for the attachment of powerful muscles which pass thence to the arms, and which, in the bat, by their contraction, strike the wings downwards in flight.

Everyone present must have observed, when carving a fowl, that there is a ridge or keel to the breast-bone, and that a voluminous mass of muscle—the breast of the fowl—is situated on each side of such keel. Now, our bat has not got such a mass of muscle on each side of the keel of its breast-bone as has the bird, and for a very good reason. In the bat, as in ourselves, the muscles which antagonise those just noticed (and which draw the arms away from the breast) are situated in the back; but in the bird, both the muscles which strike the wings downwards, and those which raise them upwards, are both together placed upon the breast, and hence its much deeper and more conspicuous keel. Still, though the muscular structure of the breast of a bat is not so perfectly arranged for flight as is that of a bird, it is an approximation to bird structure, and one we can well understand from the similarity of action. But it may puzzle some of my hearers at first to think why the mole, of all creatures in the world, should have a breast-bone at all like that of a bird. But a moment's reflection will make it obvious that the mole also requires most powerful breast muscles, in order that it may dig its way through the soil with the wonderful speed with which it does dig through it. Similar causes produce similar effects, and thus it is that the mole, like the bat and the bird, comes to have a keeled breast-bone.

The membrane of the bat's wing is a structure of extreme and peculiar delicacy as regards the sense of touch, and the perfection of this sense is doubtless contributed to by a special condition of its blood-vessels. Although the sense of touch depends, of course, directly on the nerves, the functional activity of the nerves depends upon the quantity and the sufficiently rapid renewal of the blood sent to them. This is shown by the familiar examples of numbness brought about by checking the supply of blood to any part with a ligature, as also by the increased sensibility occasioned by inflammation ; that is, through a more copious supply of blood. Now, in most animals, as in ourselves, the heart pulsates with rhythmical contractility : but the blood-vessels which distribute the blood over the body are not themselves contractile, however highly elastic they may be. In the bat's wing, however, the vessels which convey blood towards the heart (*i.e.* the veins) have been found by Dr. Wharton Jones to be themselves positively contractile, and so fitted in a most exceptional manner to help on the blood supply, thus indirectly augmenting the power of touch.

This exceptional condition of the vascular system may then have something to do with that exceptional perfection of the power of sensation before referred to, and which was experimentally demonstrated by Spallanzani. He found, not having the fear of anti-vivisectionists before his eyes, that bats deprived of sight, and as far as possible also of smell and hearing, were still able not only to avoid ordinary obstacles to their flight in strange localities, but even to pass between threads purposely extended in various directions across the room in which the experiments were made. This skill it is believed is due to an excessively delicate power of sensation possessed by the flying membrane—a power enabling the creatures by atmospheric pressure and vibration to feel, before contact, the nearness of adjacent objects. Dr. Dobson, who has paid more attention to bats perhaps than any other living naturalist, is disposed to think, and very reasonably so, that tactile power may be thus greatly increased by such increase of the surface on which tactile sensations may be received as is found in the bat's wing, and that this is the explanation of the mysterious power revealed to us by Spallanzani.

The flight of the bat compared with that of most birds is excessively fluttering; but it is a true and perfect flight, and therefore very different from the analogous action of other beasts called "flying," such as the flying squirrels, the flying opossums, and the flying lemur. In these animals the skin of



A Flying Frog.

the flanks can indeed be extended outwards to the arm and the leg, and when so stretched (as when these animals take long jumps) seems as a sort of parchment to sustain them somewhat in the air, and so far break their fall as to enable them to flit from one bough to another; but they cannot truly fly. The flying lemur is the best furnished in this respect, as it has not only a very extensive "alar membrane," but a short expansion of skin connects together not only the fingers but the toes also (which is not the case in bats), and has a true interfemoral membrane extending from the hind-legs to the tail.

There is no other such instance in beasts, or in any existing reptiles; but web-footedness is carried to such an extreme degree in a certain frog found in Borneo as to give rise to the conjecture that it was a flying animal.

Mr. Wallace, in his travels in the Malay Archipelago, encountered in Borneo a tree-frog (*Rhacophorus*) to which he considered that the term "flying" might be applied. He tells us:—

"One of the most curious and interesting creatures which I met with in Borneo was a large tree-frog, which was brought me by one of the Chinese workmen. He assured me that he had seen it come down in a slanting direction from a high tree as if it flew. On examining it I found the toes very long and fully webbed to their extremity, so that, when expanded, they offered a surface much larger than the body. The fore-legs were also bordered by a membrane, and the body was capable of considerable inflation. The back and limbs were of a very deep shining green colour, the under surface of the inner toes yellow, while the webs were black rayed with yellow. The body was about four inches long, while the webs of each hind-foot, when fully expanded, covered a surface of four square inches, and the webs of all the feet together about twelve square inches. As the extremities of the toes have dilated discs for adhesion, showing the creature to be a true tree-frog, it is difficult to imagine that this immense membrane of the toes can be for the purpose of swimming only, and the account of the Chinaman that it flew down from the tree becomes more credible."

Although no existing reptile is thus furnished, there is a small Asiatic lizard which is ordinarily spoken of as "flying," the *Draco volans*. And, in fact, though this creature cannot truly fly, but only flit, it has a membrane which can be extended from each side of the body, and which, like the bat's wing, is supported by a number of bony rods. These rods, however, are not, as in the bat, enormously elongated fingers, but are elongated ribs, which stand out freely from the body when jumping, but otherwise are folded back against the flanks.

Existing reptiles, then, present us with no close resemblance to bat-structure; but when we come to extinct reptiles—reptiles which flourished during and anterior to the deposition of our chalk cliffs—the secondary or mesozoic period—we there find reptiles to have existed which present the most striking analogies with existing bats in all that regards their modes of locomotion, and their structure as far as it is related to such modes of locomotion.

These reptiles flew, in the same way that bats do, by means of a vast membrane extending from each enormously elongated hand to the adjacent side of the body.

While, however, in the bat all the fingers of each hand are enormously elongated (to support the alar membrane)—the thumb alone remaining free—in these flying reptiles only a single finger of each hand was thus elongated, the others remaining short, and being provided with claws like the thumb.

With the approach of the winter season bats (like dormice) fall into a peculiar state of winter sleep called hibernation. For this purpose they generally assemble together in large numbers, in out-of-the-way places—caverns, hollow trees, or the roofs of buildings—hanging head downwards by the claws of their feet. During this condition the most important functions of life—breathing and the circulation of the blood—are performed only with exceedingly reduced activity, the temperature of the body becoming notably diminished.

Some of our English bats may be kept in confinement and partly domesticated for a time, small pieces of raw meat being given to them in lieu of their natural insect foot. Speaking of the long-eared bat, Mr. Bell tells us: "It is more readily tamed than any other, and may soon be brought to exhibit a considerable degree of familiarity with those who feed and caress it. I have frequently watched them when in confinement, and have observed them to be bold and familiar even from the first. They are very cleanly; not only cleaning themselves after feeding, and at other times, with great assiduity, but occasionally assisting each other in this office. They are very playful, too, and their gambols are not the less amusing from their awkwardness. They run over and against each other, pretending to bite, but never harming their companions of the same species; though I have seen them exhibit a sad spirit of persecution to an unfortunate Barbastelle which was placed in the same cage with them. They may be readily brought to eat from the hand, and my friend Mr. James Sowerby had one which, when at liberty in the parlour, would fly to the hand of any of the young people who held up a fly towards it, and, pitching on the hand, take the fly without hesitation. If the insect were held between the lips, the bat would then settle on its young patron's cheek, and take the fly with great gentleness from the mouth; and so far was this familiarity carried, that when either of my young friends made a humming noise with the mouth in imitation of an insect, the bat would search about the lips for the promised dainty."

One of the "young friends" here referred to is now the esteemed secretary at the Botanical Gardens, and he has assured me of the truth of the anecdote.

The cry of the bat is exceedingly shrill, so much so that some persons' ears are quite unable to detect it.

Homer compares the voices of the ghosts to the cries of bats. In the 24th book of the Odyssey, 6, he says: "As when bats in a corner of a great cave, when one of them has fallen from off the cluster—so they (the ghosts) went along screaming."

Or, as Pope gives it:—

> Trembling the spectres glide, and plaintive vent
> Their hollow screams along the deep descent,
> As in the cavern of some rifted den,
> Where flock nocturnal bats, and birds obscene;
> Clustered they hang, till at some sudden shock
> They move, and murmurs run through all the rock.
> So cowering fled the sable heap of ghosts.

Bats bring forth but one or two young ones at a birth—when they are received into the interfemoral membrane as into a cradle—the mother then hanging suspended not by her feet but by her thumbs.

The young are born naked and blind, and are suckled at the breast much as is the human infant.

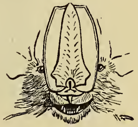
There are many kinds of bats, though their number is uncertain.

There *are* some 14 species even in England, and at least 320, arranged in some 79 genera, in the world at large.

One of our English bats, already referred to as "the long-eared bat," does indeed merit its name, since it has relatively the largest ears found in the whole animal kingdom, being about equal to the length of its entire body. They are capable of being folded up, and generally are so folded, during sleep.

Another kind of bat found in England is called the leaf-nosed bat, because in it not the ear but the nose is the seat of extraordinary skin development—productions of skin curiously folded surrounding and surmounting the external nostrils.

The use of this membrane, according to Dr. Dobson, is to serve as a tactile organ (like the wings); and this is the more probable, seeing that that family of leaf-nosed bats which is represented in England have the smallest eyes, and are devoid of a tragus or inner part of the seemingly double ear before spoken of.



NOSE-LEAF OF THE BAT *Megaderma.*

Bats are divisible into two great groups. One of them includes all the insect-eating bats (with or without nose-leaves), more or less like the bats which inhabit this country. They have almost always teeth such as those already described, often a very large tragus to the ear, and a stomach short and rounded, or at least not prolonged at its pyloric (or more specially digestive) extremity.

These bats are subdivided into various families, three of which alone immediately concern us. 1. The *Vespertilionidæ*, which

British Bats.

includes, amongst very many others, all the English bats without a nose-leaf. 2. The *Rhinolophidæ*, which includes, amongst very many others, the English leaf-nosed bats; and 3. the *Phyllostomidæ*, or leaf-nosed bats of America.

The other group of bats are made up of those, mostly of large size, called *flying foxes*, of which we have specimens now living in the Zoological Gardens. They are confined to the tropical and subtropical regions of the Old World and the Pacific, but are not found even in the hottest regions of South America. They have grinding teeth, which are not drawn out into sharp points, but have their crowns marked simply with a longitudinal furrow, in accordance with their fruit-eating habits, and their stomach (also in accordance with this habit) is much prolonged at its pyloric, or more specially digestive, end.

Certain leaf-nosed bats of South America go by the formidable name of vampires, from their reputed blood-sucking habits.

Although such a habit could only have been attributed erroneously to the entire group, one certain kind of this group is very truly blood-sucking, and its organisation is peculiarly and very strikingly modified to efficiently subserve this function.

The bat in question is called *Desmodus*, and the truth as to its blood-sucking habit has been fully established by the testimony of Mr. Darwin.* He tells us: "The vampire bat is often the cause of much trouble, by biting the horses on their withers. The injury is generally not so much owing to the loss of blood, as to the inflammation which the pressure of the saddle afterwards produces. The whole circumstance having been lately doubted in England, I was therefore fortunate in being present when one (*Desmodus d'Orbignyi*) was actually caught on a horse's back. We were bivouacking late one evening near Coquimbo in Chile, when my servant, noticing that one of the horses was very restive, went to see what was the matter, and, fancying he could distinguish something, suddenly put his hand on the beast's withers, and secured the vampire. In the morning the spot where the bite had been inflicted was easily distinguished from being swollen and bloody. The third day afterwards we rode the horse, without any ill effects."

The special modifications of structure which harmonise with this special function are mainly two. First, the form of the teeth, and secondly that of the stomach.

TEETH OF THE VAMPIRE BAT. *Desmodus*.
i, cutting-teeth; *c*, eye-teeth.

As to the teeth, the grinding ones are reduced to a minimum both as to size and number; while the two middle or cutting teeth of the upper jaw are of great size, with a sharp cutting edge well fitted to inflict the small incision needful for the animal's nourishment.

As to the stomach, it presents us with a structure unique in the animal kingdom. Here it is not the pyloric end of the stomach, but the opposite or cardiac end,

* "Journal of Voyage of Beagle," vol. i. p. 22.

which is produced into an enormously long pouch, while the opposite or pyloric end is reduced to a mere rudiment—the highly nutritious food (blood) requiring very little digestion, but needing a capacious chamber for its speedy reception.

Although this is the only bat perfectly organised to live by blood-sucking exclusively, nevertheless it is probable that various other kinds practise blood-sucking as at least one part of their mode of nutrition.

The late distinguished zoologist belonging to the Zoological Society—Mr. Blyth—has observed this habit in a leaf-nosed bat of India, one belonging to quite another family from that to which the American vampire belongs. The bat in question is called *Megaderma Lyra.* Respecting its habits Mr. Blyth tells us* as follows:—

"Chancing one evening to see a rather large bat enter an outhouse from which there was no other egress than by the doorway, I was fortunate in being able to procure a light, and thus proceed to the capture of the animal. Upon finding itself pursued, it took three or four turns round the apartment, when down dropped what at the moment I supposed to be its young, and which I deposited in my handkerchief. After a somewhat tedious chase, I then secured the object of my pursuit, which proved to be a fine pregnant female of *Megaderma Lyra.*

"I then looked at the other bat which I had picked up, and, to my surprise, found it to be a small *Vespertilio*, nearly allied to the European *V. pipistrellus*, which is exceedingly abundant, not only here, but apparently throughout India, being the same also, to all appearance, as a small species which my friend Dr. Cantor procured in Chusan. The individual now referred to was feeble from loss of blood, which it was evident the *Megaderma* had been sucking from a large and still bleeding wound under and behind the ear; and the very obviously suctorial form of the mouth of the vampire was of itself sufficient to hint the strong probability of such being the case. During the very short time that elapsed before I entered the outhouse, it did not appear that the depredator had once alighted: but I am satisfied that it sucked the vital current from its victim as it flew, having probably seized it on the wing, and that it was seeking a quiet nook where it might devour the body at leisure. I kept both animals wrapped separately in my handkerchief till the next morning, when, procuring a convenient cage, I first put in the *Megaderma*, and, after observing it some time, I placed the other bat with it. No sooner was the latter perceived than the other fastened

* In the "Journal of the Asiatic Society of Calcutta," vol. xi. p. 225, quoted in P.Z.S. 1872, p. 713.

on it with the ferocity of a tiger, again seizing it behind the ear, and made several efforts to fly off with it; but, finding it must needs stay within the precincts of the cage, it soon hung by the hind-legs to one side of its prison, and, after sucking its victim till no more blood was left, commenced devouring it, and soon left nothing but the head and some portions of the limbs. The voidings observed very shortly afterwards in its cage resembled clotted blood, which will explain the statement of Stedman and others concerning masses of congealed blood being always observed near a patient who has been attacked by a South African vampire. Such, then, is the mode of subsistence of the *Megaderma*."

Bats are most widely diffused over the surface of the globe—as their powers of flight might lead us to expect. Even Australia—so very peculiar in the character of the other beasts which inhabit it—possesses bats belonging to both of the bat families which are found in our own island.

But although the whole group of bats, and also that family to which most English bats belong—the *Vespertilionidæ*—are thus widely distributed, the geographical limits of some families of bats are very sharply defined.

To appreciate these facts it is necessary to be acquainted with the geographical areas into which the surface of our globe may be divided, each considerable tract of the earth's surface having its more or less peculiar animal population, or *fauna*, as it has its indigenous plants; that is, its *flora*. The earth's surface is divisible into six zoological regions.

1. The *Palæarctic region*, or Europe, Asia north of the Himalaya, and Africa north of the Sahara.
2. The *Ethiopian region*, or Africa south of the Sahara, and including Madagascar and also Arabia, which geologically is part of Africa.
3. The *Oriental region*, or Asia south of the Himalaya, with Southern China and the Philippine Islands and Indian Archipelago as far as the island of Baly.
4. The *Australian region*, or Australia, New Zealand, the less remote Pacific Islands, and those of the Indian Archipelago from New Guinea up to Lumbock.
5. The *Neotropical region*, or South America, together with tropical North America and the West Indies.
6. The *Nearctic region*, or temperate North America and Greenland.

Now the whole group of flying foxes is strictly confined to the tropical regions of the Old World and Australia. In the same way the family of leaf-nosed bats like those of England—the *Rhinolophidæ*—is limited to the Old World, though reaching there much higher latitudes than do the flying foxes.

The groups to which the vampires belong—the *Phyllostomidæ*—is strictly confined to the Neotropical and Nearctic regions; and the Neotropical region is not only distinguished as the head-quarters of the *Phyllostomidæ*, but also by being altogether destitute of the flying foxes and *Rhinolophidæ*.

Such being the relation of bats to space—their geographical distribution—what are their relations to time—their geological distribution?

I assume that my readers are acquainted with the fundamental facts and laws of geology, and know that the successive layers, of which the superficial crust of the earth is in very various degrees composed, are classifiable into three sets: (1) The Primary or Palæozoic rocks, (2) the Secondary or Mesozoic rocks (from the Trias to the Chalk inclusively), and lastly (3) the Tertiary or Cainozoic rocks, extending upwards from the Chalk to the present day.

Remains of beasts more or less closely resembling some of those existing now in Australia are found low down in the secondary rocks, namely in the Triassic and Oolitic formations. Generally speaking, however, beasts such as those which now exist are not found deeper than the Tertiary strata, and this is the case with bats.

The oldest fossil bat yet known is represented by a few teeth found in Eocene deposits in Suffolk. The oldest perfect fossil bat is the *Vespertilio parisiensis* of the gypsum beds of Montmartre, near Paris.

Some forms of existing beasts, however, which are *now* distinct enough, such as the ox and the pig, or the tapir and the horse, were preceded in early Tertiary times by others which were more or less intermediate in structure. This is not the case as regards bats. Bats, as soon as they appear at all, appear as thoroughly and as perfectly organised bats as are those living amongst us now.

This leads us to speculate upon questions of origin; but, before so doing, let us see that we have a clear idea of what a bat is, and can give a good definition of it.

In order that we may have this clear idea, we must consider for a few moments zoological classification.

The whole group of animals is fancifully termed the animal kingdom, in contradistinction to the world of plants—the vegetable kingdom.

This vast mass of animals is subdivided into a number of very large groups, each of which is called a subkingdom. Thus we have the subkingdom to which we ourselves belong—the vertebrate subkingdom; the subkingdom of insects, &c.; that of snails, cuttle-fishes, &c., and so on.

Each of these various subkingdoms is again divided into certain subordinate, but still very large, groups, each of which is called a class.

Thus the subkingdom Vertebrata is made up of the class of man and beasts, that of birds, that of reptiles, that of frogs, toads, and efts, and that of fishes.

Every class is again subdivided into certain subordinate groups, termed orders.

Each order is composed of families, each family of genera, and each genus of its component kinds or "species."

Now the bat, as already said, belongs to man's own class, possessing as it does all the characters which distinguish that class from the other classes of vertebrate animals.

Man's own class, Mammalia, is divisible into some dozen orders, and all the bats form one such order (*Cheiroptera*), into which no animal but a bat is admitted. The characters of this order are the possession of a truly flying membrane, sustained by very elongated fingers; and the bat is capable of being very shortly defined—namely, as *a truly flying mammal.*

Bats present no real resemblance whatever to birds, but are of course much more like ourselves (who are their class-fellows) than they are like any bird.

Similarly, in spite of this analogical relation of bats to those extinct reptiles, the pterodactyles, these creatures have no true affinity. Pterodactyles are aerial modifications of the Reptilian type, just as bats are aerial modifications of the Mammalian type. We may say, in a rough and general way, as pterodactyles are to reptiles, so are bats to mammals.

Before concluding, we may now glance at the question of the genesis or origin of bats. To those who accept the doctrine of Evolution—as I myself do—there can be no question but that bats did arise by natural generation from some anterior beasts which were not bats. But at what period, and from what progenitors? these are questions which it is quite impossible to answer at present. As has been said, there are certain cases in which we may imagine now existing more highly specialised and differentiated forms were developed from anterior less highly specialised and differentiated ones. We may do so, *e.g.*, as regards the horse and the ox. But we cannot do so as regards the bat, because up to the present time no fossil remains whatever have been found which connect bats with other creatures. Moreover, the development of the bat's wing, difficult as it is to conceive upon any view of evolution, seems to me to be especially difficult as the mere result of the survival of the fittest, when we consider the origin of the initial stages of the organ. The nearest existing relatives of the bat which are not bats are perhaps the little shrew mice belonging to the order Insectivora. Some of these are aquatic;

and it is conceivable, though there is no fragment of evidence in favour of it, that some ancestral aquatic form may have developed long fingers and webs like those of the flying frog. This speculation does not, however, commend itself to my mind as a satisfactory one; and though doubtless, could we see all the extinct forms of life which have existed during the secondary period, we should find some creatures developing by more or less rapid stages along a definite course in the direction of the type of structure selected for our consideration to-day, and though I am ready to make an act of scientific faith in the existence of such creatures, I confess my imagination fairly baffled in its attempts to depict them, or the road which this particular course of evolution followed. We must wait patiently for more light from palæontology. But we may wait very hopefully. We may do so because the wonderfully rich harvest of fossil remains now being gathered in North America supplies us with good and solid ground for hope.

Already forms have been discovered there so strange that they cannot be satisfactorily grouped in any existing order of mammals—forms such as imagination could hardly have anticipated. We may, then, not unreasonably expect that sooner or later—perhaps very soon—fossils deeply buried in the secondary rocks will come to light, clearly pointing out the line which has been followed in the evolution and development of the only truly flying mammal—the bat.

EXPLANATION OF PLATE CXXXVI.

Fig. 1. A Flying Fox from Samoa, *Pteropus Whitmeei.*

Fig. 2. Skeleton of a Flying Fox.

Fig. 3. Side view of breast-bone of ditto.

THE EIGHTY-TON GUN, OR WOOLWICH INFANT.

By Captain C. O. BROWNE, R.A.

[PLATE CXXXVII.]

THE appellation of the "Woolwich Infant" was originally given to the first 35-ton gun. We are now about to consider a "new arrival" of not only vastly greater power, but one which, starting in certain respects, as it does, on new conditions, may be regarded as the herald of a race of giants.

The circumstances which have called the 80-ton gun into existence are those under which the struggle for supremacy between guns and armour is now carried on. It becomes necessary, therefore, to glance briefly at the stage of development to which this branch of warfare has been pushed.

Our first armour-plated vessel, the *Warrior*, was commissioned about 1861. She carried $4\frac{1}{2}$-inch plates on the greater part of her sides, with 18 inches of teak backing and $\frac{3}{4}$-inch inner skin of iron. This was found to resist fairly the attacks of 7-inch Armstrong breech-loading guns and 68-pounder smooth-bores, except at very close distances. Great strides were shortly made in the manufacture of guns; but at the same time the naval constructors arrived at such forms and designs for vessels as enabled them to carry much heavier armour, decreasing the surface of the side to be plated by lowering the freeboard, by shortening the entire vessel, and further by greatly diminishing the size of the batteries requiring to be surrounded, as it were, with an iron wall. This was done by drawing the guns into a centre battery, or placing them in turrets on Captain Cole's system. Thus it has come to pass that in our fleet, at this present time, we have on the one hand the *Warrior* and the *Minotaur* or *Northumberland* class of ships, with $4\frac{1}{2}$ inches of iron and 7-inch (Woolwich) guns of $6\frac{1}{2}$ tons weight, and on the other hand the *Devastation*, *Thunderer*, and *Glatton*, with 14 inches of plate and 35 and 38-ton guns; while the *Inflexible* is now being constructed with 24 inches of armour laid on in two plates of 12 inches each, and is to carry 80-ton guns. So rapid a development of power is obviously alarming in more than one respect; for not only are we compelled to devote enormous sums of money to the construction of such designs, but also the new vessels so rapidly dwarf and outmatch their predecessors in their means of attack and defence, that there must necessarily exist in the armament of each nation a few ships which are able to dispose of all comers except the individuals of about their own class, who might probably be a number that might be counted on our fingers. An uncomfortable element of uncertainty is thus brought into the question of naval warfare, for the same conditions obtain in the fleets of all formidable naval Powers. Suppose that England, in the *Inflexible* and *Dreadnought*, has vessels that are decidedly more powerful than any of her enemy's fleet. Suppose even that the next class of ship, the *Devastation* and *Thunderer*, are sufficiently powerful to be expected to hold their own fairly against the most formidable antagonist, which we shall see hereafter is not the case. What guarantee have we that vessels, say such as the *Monarch*, may not be called upon to engage with that antagonist? So that a ship which we look upon as rather a powerful ironclad may after all be placed in circumstances when she may be called upon to contend against an invulnerable antagonist whose guns, under favourable conditions, are capable of piercing her own armour with ease. This danger is seen to be the greater when the difficulty of identifying an antagonist is taken into account. One thing at all events is clear, and it is that which at present concerns us. Our vessels must be supplied with the most powerful guns we can make. Hence the importance of the 80-ton gun and its kindred. The particular desideratum demanded of the 80-ton gun was, that it was to be capable of penetrating 20 inches of iron, firmly backed, at 1,000 yards range. This there is reason to hope is likely to be realised, as we shall see hereafter, but we have first to deal with what has actually taken place.

The gun itself (*vide* Pl. CXXXVII. fig. 1) is made on the Woolwich system as perfected by Mr. Fraser; that is to say, the centre portion or "A tube" is made of a block of cast steel supplied by Messrs. Firth. Round this are "built up" five cylinders formed of coiled wrought iron, and a large mass termed the trunnion hoop, which are attached in succession in their proper places by means of heating them so as to expand them and enable them to pass on, and then in cooling to grip the interior portion firmly. The breech end behind the base of the steel tube is closed by a block of wrought iron screwed into the posterior portion of the "breech-piece" coil. Those who are acquainted with the manufacture of the Woolwich guns will recognise that this amounts to saying that

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Fig. 1.—The Gun.

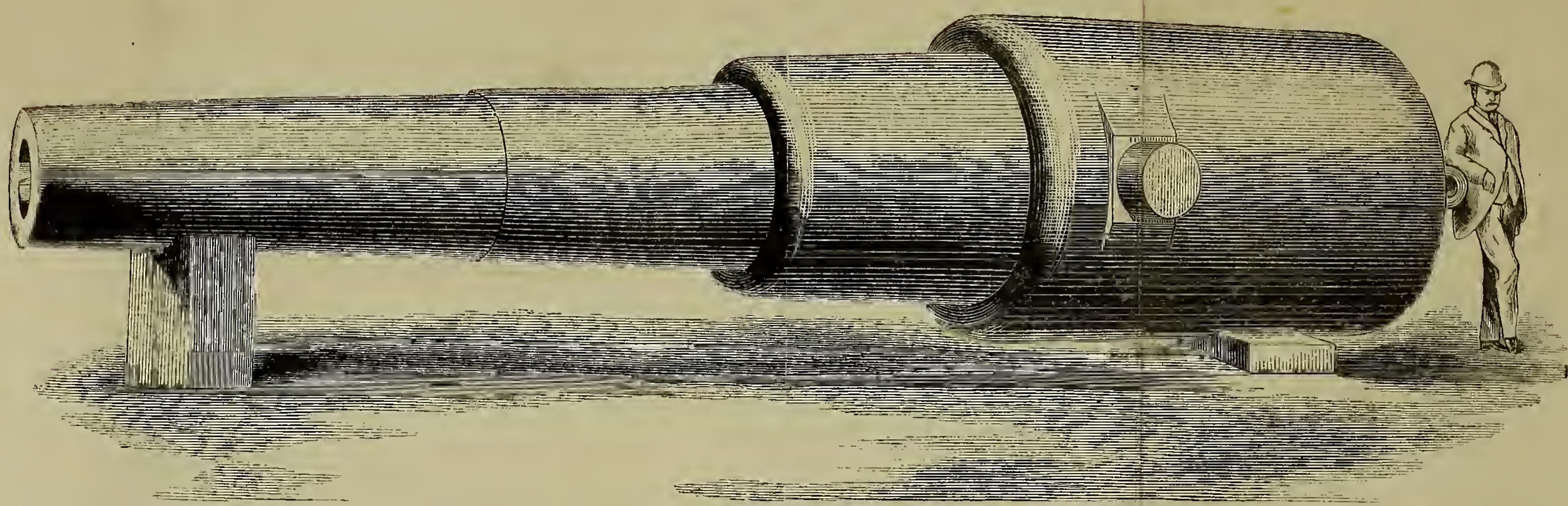
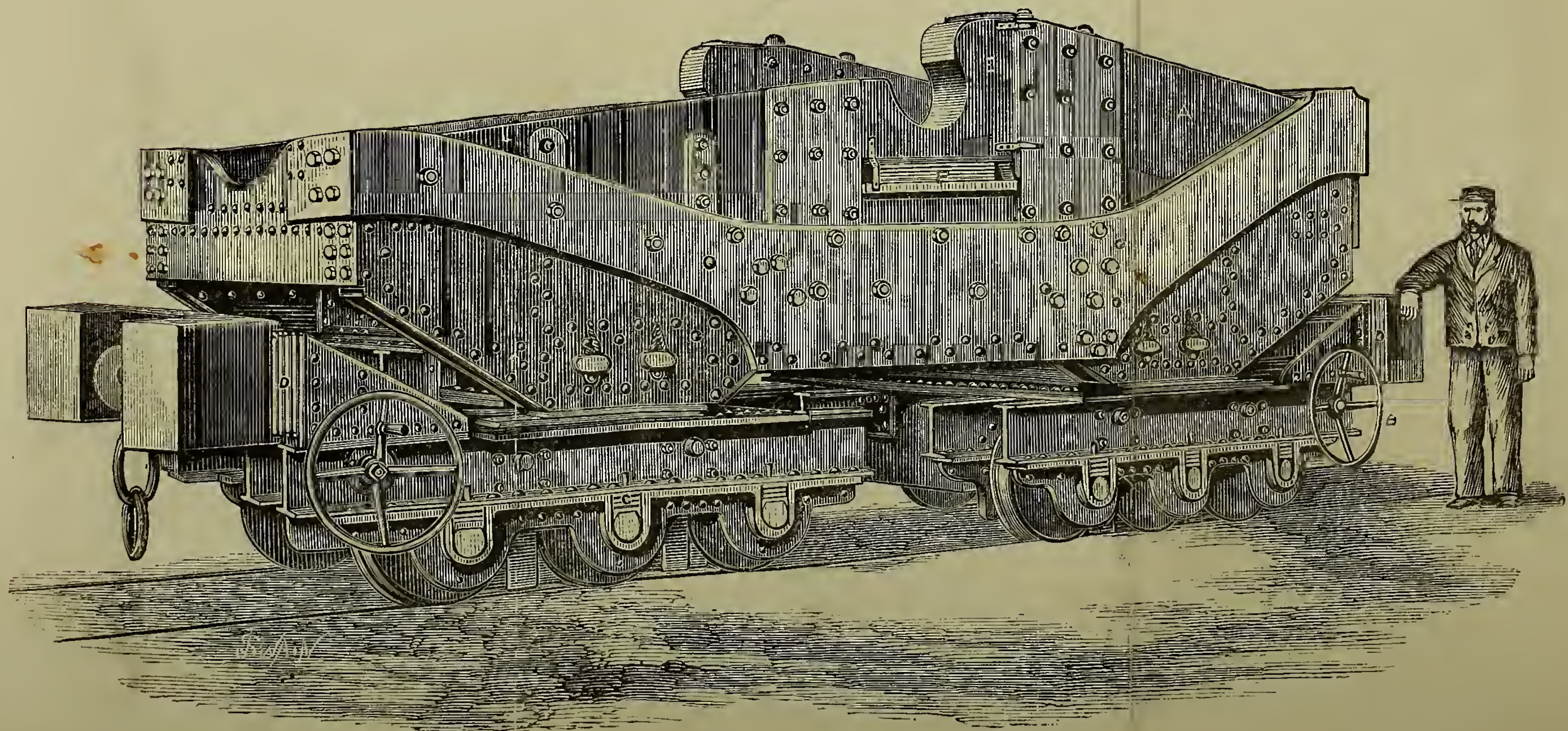


Fig. 2.—The Carriage.



THE WOOLWICH INFANT.

this piece is constructed on the Fraser system. The steel tube is roughly bored out before the gun is built up and finished, and rifled afterwards. The total weight of the iron forgings and other separate parts to be thus united is given by Major Maitland, the Assistant-Superintendent of the Gun Factories, in a paper sent to the R.A. Institution, as 164 tons 16 cwt. when in the rough state.

In the manner just described a gun can be made, so it is believed, as strongly as is possible on any system. It is difficult to speak positively, because steel guns are frequently found to be very strong, and when tested to destruction they have exhibited greater durability than was thought could have been shown by any other material. Possible or probable durability of a high character, however, is not at all so desirable an element to exist in a gun as the *positive assurance* of some standard, even though a much lower one. This is what may be claimed for wrought-iron guns compared with steel. The former are very reliable, and possess very high resisting powers; the latter may possess extraordinary powers, but may occasionally burst unexpectedly, and when they do so the fragments fly violently in all directions; whereas should the wrought-iron gun give way, it does so gradually, and so no danger is to be anticipated. The conditions which principally have to be considered, at all events with the Woolwich guns, are those under which the bore of the gun may best perform the work required of it, and resist the effects of wear and tear caused by the erosion of the gas, our guns having worn out in many instances very rapidly. Matters have been improved by the introduction of tight-fitting copper gas-checks, which fix on to the base of the projectiles, and prevent the gas from escaping past their sides, and so scoring the bore of the gun as well as wasting available power. It has been considered desirable, however, that under any circumstances the pressure of gas in the bore of the gun should not be allowed greatly to exceed about 25 tons per square inch. The principal problem involved in the whole question may be thus stated. How are we to discharge the projectile with the desired velocity without bringing on the bore of the gun a pressure exceeding about 25 tons? The size of the bore of the gun affects the question, because much depends on the space it allows the charge to occupy. A larger bore admits of a shorter cartridge, and brings the powder into a more compact mass through which the flame of explosion passes more easily, and so generates the gas more rapidly. Connected with this is the question of giving guns enlarged chambers, so as to burn the powder in a more favourable manner than the desired size of bore would allow. Sir J. Whitworth, in 1872, had great success with a breech-loading gun with an enlarged chamber, which was probably the first that was tried by anyone. An enlarged chamber with a muzzle-loading gun is rather a more difficult matter to arrange, because in many cases the cartridge has to be made to set up and expand in order to enter the enlarged chamber properly.

A much more complicated question than that of the size of the bore is that of the powder. To understand this, it is necessary to be clear generally as to what takes place in the process of explosion. The word "generally" is used advisedly, for it would be rash for anyone to profess to know all that takes place. Speaking generally, then, the explosive gas commences to be generated by the flash of the tube igniting the surface of the adjacent grains of powder, and the flaming gas so generated rapidly expands in all directions open to it; and thus, under the pressure of close confinement, rushes violently through the interstices between the grains of the entire charge, which grains, becoming ignited, burn from their exterior surfaces towards their centres. Thus the gas developed rapidly acquires sufficient power to drive the projectile up the bore, following it up with all the force it acquires from the further generation of gas as the grains burn through. Thus it will readily be seen that a small grain of powder facilitates rapid explosion by burning through very quickly, and in a small charge of powder gives the greatest effect. As the charge increases, however, there soon comes a point when the interstices between the grains become insufficient for the passage of the flame, and, in fact, the bore becomes choked, so that a larger grain becomes necessary, even where speed is the sole condition to be considered. On the other hand, too large a grain is blown out partially consumed from the muzzle, and so partly wasted. Nevertheless, so great is the evil of violent action, that it is better to err on this side than on the other. A committee was specially appointed to investigate the action of powder in the bore of a gun, who made a series of experiments in 1870, showing by means of Capt. Noble's chronoscope and plug or pressure gauges the velocity of the projectile and the pressure of gas at various points in the bore of a gun. By their labours they arrived at a form of grain which gave a prolonged and continued pressure on the projectile throughout its passage along the bore, without at any one instant causing an excessively high maximum pressure, such as would strain the gun disproportionately. This kind of powder necessitates an increased charge as compared with that which burns more rapidly, because the latter acts at greater advantage by performing its work when the space behind the projectile is smaller. It is much better and cheaper to use more powder than to injure the gun, and so the slow-burning powder is preferred. Obviously, however, it is impossible so to regulate matters that the powder is exactly burnt up as the shot arrives at the muzzle; and were it so, the pressures, as the projectile got near the muzzle, would be very small, because the burning surface and quantity of gas generated becomes less and less as the grain becomes smaller. Hence it is easy to see the difficulty of avoiding the escape from the muzzle of partially burnt grains of powder, undesirable as that may be.

The word "grain" has been hitherto used on principle; but the form that the grain takes in the charges of the largest guns is that of a rough cube with an edge from $1\frac{1}{2}$ to 2 inches in length, each single grain, or "pebble," as it is called, containing in itself a considerable charge of powder. So far the size of the powder-grain only has been considered. The density, and even the glazing, have also to be taken into account. The density indeed entirely alters the conditions, for not only does powder burn more rapidly as it is less dense, but also, it has been recently shown, the assumption that the grain or pebble burns evenly from the outside to the centre is quite untrue generally, for the pebbles burn through in certain places and become honey-combed or sponge-like during combustion, in all powders of ordinary density, and are blown out of the gun, and may be picked up afterwards in this condition.

The question of the powder has been discussed at some length, because it is necessary to know something of the nature of the problem to be worked out, in order to understand the reason for the long series of experiments that is being now carried on in the Woolwich Arsenal.

In the design of the ammunition there is nothing differing essentially from that of other large guns except the magnitude. The projectiles are eventually to be of the same general form as our other service projectiles. The armour-punching shot or shell have ogival heads and sharp points, and are made of chilled iron on Sir W. Palliser's system. The section of such a head is in the form of a rather pointed gothic arch, and the metal in the head is chilled white and very dense and hard, while that in the body is mottled. Projectiles of this kind have extraordinary powers of penetration; the metal in the head being of the kind to stand up under the compression of impact, and to cleave its path into the soft iron of which armour is necessarily composed, while the base has more tensile strength and holds well together. That the head has the qualities of intense hardness is seen in the fact that the point never flattens, but remains sharp to the end, while the crushing strength of the metal is apparent from the fact that it remains quite cool after impact, while the pieces of plate that have suffered compression are for a considerable time intensely hot.

The final calibre of the gun is intended to be 16 inches, and the rifling consists of eleven grooves in the bore at a spiral increasing from nothing, or the direct direction, at the bottom of the bore to one turn in 35 calibres at the muzzle. The projectiles have three gun-metal studs for each groove. The weight of the projectile, when the bore of the gun is of its full dimensions, may be about 1,600 lbs., and that of the charge 300 lbs., with a muzzle velocity of about 1,400 feet per second.

The question of the carriage is an interesting one. It was clearly desirable to find some way of facilitating the operations of moving this gun about. Light guns travel on their firing carriages. Heavy guns are generally conveyed entirely separate from their carriages. Indeed their carriages are massive frames of iron, supplied only with small trucks, which are brought into play by hydraulic lifts when the carriage is to run up a few feet along its platform. The carriage and gun are separately lifted by cranes, sheers, gins, and the like; and by the same means the gun is placed on its carriage when mounted, and lifted off and lowered when dismounted. These operations are slow, and such as need much mechanical apparatus, and considerable skill to carry out with safety to those engaged in them, because with exceptionally heavy weights the strain likely to be brought on every part of the gear must be carefully calculated. A weight of 80 tons would be so greatly in excess of any that has hitherto been dealt with, that a special supply of apparatus might have to be made to any place where the gun was to be moved and mounted. Even at the Royal Arsenal the great hydraulic crane, which was hitherto found able to seize up and swing round any desired weight as if it were playing with it, had to be strengthened. The operations of loading and unloading, mounting and dismounting, could not fail to involve considerable expense and difficulty at out-stations. Under these circumstances the idea was suggested of dispensing with them by building a carriage on which the gun might both fire and travel; in fact, to make the monster approach in its conditions to a field piece—with this exception, that its movement was always to be on railway lines, for which purpose the carriage must be made to suit the narrow gauge. Whether this idea originated with the Royal Carriage Department or with Major Maitland of the Gun Factories, it may be hard to say; at all events it is to be highly commended.

The carrying out of it was, however, by no means easy, and Colonel Field and Mr. Butter are to be congratulated on their complete success. To understand the difficulties, the action of a gun in firing must be considered. The carriage is driven back by a violent force applied at the bottom of the bore of the gun. The recoil is then checked gradually by brakes applied to the trucks. The direct strain the carriage must, under any circumstances, bear; but this is not what it generally suffers from, but rather the contortion that it receives, from the fact that the centre of gravity of the mass cannot be brought opposite to the bottom of the bore of the gun, but must fall below it. A violent horizontal blow backwards, opposed by a resistance in a lower plane, causes obviously a contorting "couple" or twist to be generated, driving the posterior part of the carriage against the ground, and, as a secondary effect, causing the front of it to lift or jump. In our modern heavy gun-carriages this has been brought to a minimum by getting the gun well down between the brackets or cheeks of the carriage, and making the latter as low as possible, so that the bottom of the bore, the centre of gravity of the mass, and the surface on which the carriage slides as it recoils, are brought as nearly into the same plane as possible. With the 80-ton carriage (*vide* Pl. CXXXVII. fig. 2) the evil could not be got over in this way; for the gun-carriage trucks, which were to be also railway trucks, must be capable of running along rails, and round any curves that might exist. The carriage was necessarily very long, the length of the gun itself being 27 feet. Consequently, the only way in which it could be made to turn, was to construct it in the form of an upper carriage resting on "bogies" (*vide* fig. 2). Each of these bogies being attached to the carriage by a single centre pivot, the whole might run round any curve that the bogies could take independently, the carriage adjusting itself like a flexible connection. This, however, necessitated the gun being a considerable height above the trucks, and so called for great strength in the carriage, which would be severely taxed by a downward contorting blow on firing. The carriage is made of iron, except a wood block termed a buttress, which supports the trunnion-hole block at A, and a few other minor parts. Strong india-rubber springs, or buffers of sheet india-rubber an inch thick, are laid in between pieces of iron plate at C C, above the trucks, to prevent shocks or jars in travelling. Hand-wheels are fixed on the front and rear of the ends of the front and rear bogies respectively, which, by means of bell-crank gear, bring double wood brakes to bear on all the trucks. The platform of each bogie consists of longitudinal pieces of angle iron, 12 inches by 6 inches, with $1\frac{1}{2}$-inch web. The carriage rests on gun-metal bearings on the bogies. From what has been said it may be seen that, on firing, a violent backward and downward blow will be given to the carriage through the trunnion blocks at E and F. Here again is india-rubber inserted under pressure, to act as a cushion. It may be seriously doubted how this would do in hot climates. For the service for which the carriage is at present intended, however, it serves its purpose very well. For proof firing the rails were made to incline upwards to the rear, so that the recoil of the gun would be checked, and the piece easily run up ; or it might be made to return immediately into the firing position. When once the gun is mounted on this carriage, there is no necessity for dismounting it. It can travel to any part of England, and be brought into action wherever it may be desired, so long as the ordinary narrow gauge line (4 feet $8\frac{1}{2}$ inches) exists, laid on a permanent way of sufficient strength to carry the gun and carriage, whose total weight will probably be finally about 118 tons.

The gun was at first only bored out to a calibre of $14\frac{1}{2}$ inches, in order to obtain some experience as to the burning of the powder and the behaviour of the projectile before the final dimensions were given to the bore. It was brought down to the proof butts in Woolwich Arsenal, and fired for the first time on September 17 last.

On this occasion six rounds were fired with a projectile of 1,160 lbs. weight, and charges of pebble powder composed of cubes of $1\frac{1}{2}$-inch edge, the weight of the charge gradually increasing from 170 lbs. up to 240 lbs. There is no occasion to enter into the exact details of each round ; it is preferable to keep to the object and general results obtained. As mentioned above, the pressure in the bore of the gun was to be kept, if possible, nearly about 25 tons per square inch, and the velocity was to be brought as high as possible consistently with this condition. It has been already explained that the velocity is obtained by measuring the time occupied by the shot in passing over the space between two screens, a current of electricity being broken or interrupted momentarily as the shot cuts the wires of each screen. The pressure was measured by pressure gauges fixed in the base of the projectile and the bottom of the bore of the gun.

The charges were 170, 190, 210, 220, 230, and 240 lbs., which gave pressures on the ball of the shot of 19·4, 18·2, 19·8, 21·4, 21·8, and 27·3 respectively, with 24·2, 23·3, 24·8, 22·2, and 29·6 on the bore for the first five rounds. The velocities at the muzzle were 1,393, 1,423, 1,475, 1,503, 1,550, and 1,550 feet per second. These must be considered good, although as yet the weight of the projectile was but small. A velocity of 1,550 feet is a very high one for a rifled shot. Smooth-bore guns occasionally fire round shot at higher velocities ; but the projectile is very light compared with the charge, is easily projected, and loses its velocity rapidly. Those unused to these sort of statistics may form a good idea of the speed of a gunshot, from the circumstance that an enemy's shot comes to you rather before the report of the guns. You see the flash, but the shot has whizzed past you and done its work so far before you hear it. That this might be expected is obvious from the fact that the speed at which sound travels is not greatly in excess of 1,100 feet per second; so that for it to overtake the shot a very long range and considerable elevation is required. The pressure against the base of the projectile would naturally be somewhat less than that against the bottom of the bore, because the shot is fast moving away from the gas; but it is a little difficult to explain why the difference is quite so great as it is.

We may run briefly through the succeeding trials of the gun, which have been in pursuit of the same ends as the first; that is to say, the object has been gradually to feel the way experimentally to the condition under which the greatest velocity can be given to the shot without subjecting the gun to a pressure greatly exceeding 25 tons per square inch.

On Nov. 18 and 19, and Dec. 9 and 10 last, fifteen rounds in all were fired, which concluded the trial of the gun at its first calibre of 14½ inches. In March it was again fired, when the bore had been increased to 15 inches; and in May, when the powder chamber had been increased to 16 inches. The general results of these trials have been to show that the powder is better, or at all events more uniform, in its effect as the density is slightly increased above the average; that the employment of a charge over 250 lbs. was unprofitable and wasteful until the calibre arrived at 15 inches; and that the best results, not only those which are least injurious to the gun, can be obtained without increasing the pressure to 30 tons. Of course, as the calibre increases, the power of burning powder to advantage does so too. Probably a 2-inch cube or grain of powder might be employed to greater advantage when the calibre is brought up to the full amount of 16 inches, and the chamber still further enlarged. At present experiments are being continued with 1½-inch cubes, but very promising results were obtained with those of 1¾ and 2 inches during the trials. A projectile of 1,460 lbs. weight has been fired. The enlargement of the chamber appears to have good results, but a sufficient number of rounds have not as yet been fired to enable a certain opinion to be given.

This is the first heavy gun whose chamber has been enlarged; but some successful experiments have been made with field guns in the Royal Arsenal recently. It may be observed we are not as yet finally committed to an enlarged powder-chamber, for the enlargement has at present been limited to the intended final calibre of the gun.

The greatest velocity that has yet been obtained with the 80-ton gun is about 1,550 feet per second.

To pass on to the more interesting question of the probable power and uses of the gun on service.

The most important matter is the power to penetrate armour.

It is obvious that there is no use in one ship directly attacking another whose plates are entirely beyond the power of any guns that can be brought to bear on her; there are other questions, however, that are not so easily disposed of. For example, when two ships engage, as we may assume will commonly occur, who have the power, under favourable circumstances, of piercing each other's sides, how far will a more powerful gun enable a vessel with thinner armour to hold her own by engaging at longer ranges than suits her adversary? This is important, as affecting the question of what benefit our lighter-plated vessels would derive if we put more powerful guns in them. The matter that concerns us chiefly, however, is, first, under what conditions would a heavily-plated ship, such as the *Inflexible*, go into action against vessels now afloat, or against such as are known to be building. Of vessels at present afloat, the most powerful in our own navy are the *Devastation*, *Thunderer*, and *Glatton*, carrying 14-inch and 12-inch armour and 38 and 35-ton guns, throwing 600-lb. shot.

The ships carrying the thickest armour among the navies of foreign Powers that we can hear of as at present afloat are, the Russian circular ships, *Admiral Popoff* and *Novgorod*, the former carrying plating and iron girders, &c., which Mr. Reed estimates as *equivalent* to 18 inches of iron, and the latter armour equivalent to 11 inches. With the 12 inches backing and edge plates of the 1¼-inch skin, our 14 inches of armour, which includes the bare thickness of the front plate, is probably quite a match for the former, and probably the guns of the *Popoff* are not nearly as powerful as the 35 and 38-ton guns in our ships. Russia is, however, building a notable ship, the *Great Peter*, which is to carry 15 inches of armour and 40-ton guns. She, therefore, ought to be rather more than a match for the *Thunderer* whenever she is afloat; but there has been much delay and disappointment, and it may be questioned whether we are justified in assuming that she will come up to this standard in all respects. The Turkish navy has ships with 12 inches of armour and 9 inches of armour. The French have nothing afloat with more than 8½ inches and 21-ton guns; but they have vessels building with 15 inches and 12 inches armour and 21 and 35-ton guns. The Italians have two enormously powerful ships building, the *Duilio* and *Dandalo*, which are to carry guns of 100 tons and armour of from 22 inches to 16 inches, we believe, on various parts.

Probably, then, when the *Inflexible* comes into the service with armour of from 24 to 18 inches, and 80-ton guns, she will find two Italian ships whose guns ought to be more than a match for hers, and whose armour is but little inferior. Until trials take place with 100-ton guns, we are not in a position to speak of them; but, being made by Sir W. Armstrong, they will presumably be the same class of gun as the 80-ton, but more powerful in proportion to their increased size. It is to be presumed from the trials that have taken place that the 80-ton gun may not fall short of expectation, and may prove able to pierce 20 inches of armour up to about 1,000 yards range. It ought, therefore, under favourable circumstances, to be capable of piercing the sides even of the *Duilio* and *Dandalo* at short ranges; the question is whether the latter could hold off so as to increase the range so much as to retain the power of penetrating even the thicker sides of the *Inflexible* with the 100-ton guns, while the distance was such as to save her own thinner armour from the 80-ton guns of the *Inflexible.* This would be a difficult matter, for the larger the gun and projectile, the less any increase of range tells against the velocity of the shot; so that even where it was practicable to secure the desired range, it is greatly to be questioned whether the gun could be made to hit the desired object. The uncertainties are greatly complicated by the fact that a vessel which could present her plates at an oblique angle to the blow of an enemy would obtain a great advantage. Of the other vessels afloat, the *Great Peter* might be looked upon as the most formidable. Of her 40-ton guns we can only judge at present by comparison with our own 35-ton guns, for the 38 has never been tried against plates. Now the 35-ton gun has just got the head of its shot through a target which had as much as 17 inches of iron, besides skin and backing, at 70 yards range. But this target has always been considered by the Admiralty and other authorities an imperfect one; its plates were divided into three thicknesses, and altogether it is more than questionable whether the 40-ton gun could get through the *Inflexible* at her weakest place, even at the shortest range. Probably the *Inflexible* would be invulnerable to every foreign ship afloat, except the two Italian ones; and that she could pierce the sides of all except these at any range likely to be desired, there can scarcely be a question. We have, then, the case before us of three ships coming into the arena of naval warfare, who, even at this stage of development of guns and armour, stand in almost the same relationship to all ironclads previously afloat as they in their turn bore to wooden ships. The question raised by such a sudden increase in power as we naturally connect with the 80-ton gun is this: Are we proving the possibility of the construction of armaments on such a scale that there is no definite limit not only to the power to which guns and armour may eventually be brought, but to the augmentation that may *suddenly* be made in them? If, for example, the success of the 80-ton gun is only to be regarded as an illustration of the proof of the declaration that a gun of 200 tons could be made, and armour of 20 inches proves that armour of 36 inches could be provided, it appears that it lies in the power of any nation, who will pay the required price, to construct an invulnerable vessel that might destroy everything at present afloat by means of her artillery. The rapid development of armour, at all events, favours the use of torpedoes and other means of attack below the water line, and the shape that naval warfare may at any future time assume seems more uncertain than ever.*

* The blocks employed in printing the plate have been kindly lent by the publisher of the "Engineer" newspaper.

AQUARIA: THEIR PRESENT, PAST, AND FUTURE.

By WILLIAM ALFORD LLOYD,

Manager of the Crystal Palace Aquarium.

EIGHTY-SIX years ago—in the year 1790—there might have been seen trudging along the streets of Edinburgh an "old blue-coated serving-man," carrying an earthenware pitcher or jar, of three or four gallons capacity. That pitcher contained sea-water for the marine aquarium of Sir John Graham Dalyell, Bart., who thus employed a man, or probably a succession of men, from the time he began aquarium-keeping till he finished at his death in 1851—a period of sixty-one years. The jar was sent to the sea to be filled twice or thrice weekly; but averaging it at five times a fortnight, and allowing four miles for each double journey from Great King Street to the sea and back, that amounted to 39,650 miles from the year 1790 to the year 1850, which was an enormous and perfectly needless expenditure of force, expressed in time and money, even although the results of Sir John's investigations were given to the world in five such important quarto volumes as his "Rare and Remarkable Animals of Scotland," 1847–8; and his "Powers of the Creator displayed in the Creation," 1851–8.

Dalyell's mode of operation, as told to me by his sister Elizabeth, in two letters dated 1860, and printed in the "Zoologist" of Nov. 1873, vol. viii. Second Series, pp. 3757–8, was as follows:—He kept his living marine animals, consisting of the lower kinds below fishes, in a number of glass cylindrical jars, of various sizes and proportions, and with usually one animal in each. The water in these jars he changed every morning, "often twice a day, if he perceived the smallest fragment amongst it, wiping and washing the glasses very clean." He then threw away the water so used, and replenished it from the earthenware jar with the water got from the sea. At one time I should not have termed this aquarium-keeping at all, because of the change of water. (See "Crystal Palace Aquarium Handbook," 1875, p. 7.) But now, having got to think more broadly, I recognise this, not as a change of water in the sense of its being lost, but merely as a change of position from a house in Edinburgh to the sea, and back again. That is to say, the water he dismissed from his jars went into a gutter in a street, or into a sewer below it, and found its way by gravitation into the ocean again. Or, if it were poured on the ground into which it soaked, it found its way back to the sea by an infinitely more circuitous route. But, had Dalyell been more of a general philosophical thinker as well as a naturalist, he would have saved himself this very great amount of cost and trouble. Had he but reflected on that which was then known, namely, that water—both sea-water and fresh water—is practically indestructible, and that any decaying organic matter, animal or vegetable, or both mixed, can be got rid of, and the water be left pure, then he would have saved his servants their weary walks of more than as far, in their aggregation, as twice round the world, nearly.

In the ocean of course various animals and plants are incessantly dying in large numbers, and their decomposing remains are prevented from permanently poisoning the water in which other animals live and breathe, by the incessant motion to which the sea is subjected, and this motion brings the water into purifying contact with the atmospheric air which everywhere exists. It is this air, or rather the oxygen in it, which the water takes up in greater quantity than the nitrogen, which is another and larger component of the atmosphere, which is the source of purification alluded to, the water being merely a medium or a vehicle for the exhibition of the oxygen. In addition to this, vegetation grows by the action of light, and decomposes the poisonous carbonic acid gas evolved by the breathing of animals, the carbon being used to form the woody substance of the plants, and the residual oxygen being liberated for the use and benefit of the animals. Thus the ocean, and rivers, and lakes, and all other waters in nature, of varying degrees of freshness and saltness, by motion and vegetation, both originating from the sun, are maintained sufficiently pure and respirable.

These operations were going on almost at Dalyell's door, yet he did not learn to apply them to practice, as he might have done. What he did was this: He fed the animals in his jars on mussel flesh, which is easily diffusible in water, and which quickly makes it milky; and this, with the absence of growing vegetation, and the breathing and other emanations of the animals, soon caused the water to become offensive in appearance and in smell. So he threw it away. But the very act of pouring it, and the motion of it as it trickled onwards to the sea, purified it, because such an act was an unconscious imitation of what nature does. Had Sir John but thought of the merely vehicular character of water, and of its incapability of being decomposed save by a very slow and expensive process, he would at once have seen that the minutely disseminated mussel flesh and its juices in the water made that water unfit to support life, only temporarily. It was not the water itself that was not fit; it was only something in the water that was wrong, and if that something were removed the water would be left as good as ever. If, therefore, instead of sending it back into the sea by a long road, and then going to the immense pains to dip it back again, he had poured it into a large receptacle in his own house, such receptacle or reservoir being many times larger than the aggregate contents of all his glass jars, he would have found that in a short time he would have possessed a source of supply for the jars quite as good as the ocean provided. Had he, in addition, placed his reservoir in a cool cellar, and had a pipe connecting it with the study to which Miss Dalyell has incidentally alluded, with a funnel at the upper end of the pipe, in which was placed a piece of straining-cloth or a small hair-sieve, to arrest the coarser pieces of decaying organisms, and if he had poured the water he had used into this funnel, the arrangement would have been still better. Yet better would it have been had he possessed another pipe leading upwards from the reservoir, through which he could pump up the sea-water as he wanted it. Best of all would have been some form of incessantly-working machinery, by means of which the water would be always coming up day and night from this large and cool reservoir into the experimental glasses, for then they would have been constantly kept at an even temperature and in a state of constant aeration. This would have done away with the necessity of the everlasting wiping and washing of the glasses; and, they being thus left alone, and in a certain amount of daylight, vegetation would soon have appeared in them, stimulated by the action of that light, without having been visibly introduced, but present everywhere in the seeds or spores of plants, merely waiting to be developed. Such an arrangement, indeed, would have been precisely that of the best modern aquaria as now made, in which the water is so continually and abundantly aerated by ceaselessly moving machinery, that impurities have no time to accumulate, but are oxygenated and dissipated as quickly as they form. In the Brighton and Havre public aquaria, the old and intermittent system used by Dalyell has been reverted to, and of course with ill results, as the water freshly obtained from the sea is turbid when seen in large masses, and is unhealthy for the animals, only a small number of which therefore can be kept in great bulks of fluid, because it is insufficiently aerated. This will be the case also at the Scarborough aquarium, now being built on the same erroneous principle.

Dalyell, however, was no mechanician or physicist, and he knew nothing of marine botany; so he just did as his neighbours did with their fresh-water gold-fish globes—he changed the sea-water and threw it away as quickly as it became sullied, and this water he obtained at no great cost, he living close to the sea. Or if the cost of time in getting it was considerable in proportion to the work done, *i.e.* the quantity obtained, it mattered not much to him, as he was a rich man. Yet, had he but known it, the sea-water he thus obtained was less good for the animals he kept than it should have been, inasmuch that it was from the adjoining Firth of Forth, and of the density of but 1·024, at a temperature of 60° F.; whereas had he kept it for some months, it would have evaporated to the more proper density of 1·027 at 60° F., taking distilled water as being 1·000 at 60° F.

I have given this narration as showing the state of things aquarially at the end of the last century, and during the first half of the present one, and also as being the mode of operation which the general public, and even the great mass of the higher and better educated classes of society, still believe to be the system necessary to be followed in the maintenance of aquaria.

In the year 1842 the late Dr. N. B. Ward published the first edition of his book, in 8vo., on the growth of plants in closely glazed cases, and this in 1854 was followed by the second edition, in 12mo. In 1853 Dr. N. B. Ward's son, the present Dr. Stephen H. Ward, gave a lecture on this subject at the Royal Institution, which was published as a 12mo. pamphlet in the same year. All three of these are now and have been long out of print, and they bear testimony, indubitably, that N. B. Ward experimented with aquaria about the year 1840, though he did not use the word "Aquarium," which was employed for the first time in print, as far as I know, twice by Mr. P. H. Gosse, in his "Devonshire Coast," post 8vo., 1853, at pages 234 and 441. That is to say, N. B. Ward is the earliest recorded person who *intentionally* arranged together certain animals and plants in water, so that these two sets of organisms should mutually and partly support each other, the plants giving off oxygen and taking up carbon, and the animals taking up oxygen and giving off carbon, thus decomposing and rendering harmless the carbonic acid gas as continually as it was evolved by the animals, and maintaining the water pure. In Dr. S. H. Ward's pamphlet, just named, is a long, circumstantial, and most interesting narrative of how Mrs. Anne

Thynne did the same thing precisely with sea-water and marine animals and plants. This lady being in London in the year 1846, and having some living corals and sponges, used to send occasionally to the coast for supplies of water for her creatures. But finding that if a quantity of this water were taken up in a jug and let fall again from its spout in a slender stream, it lost whatever impurity it contained from contact with air in this much comminuted state, she ceased to get more from the sea, and instead got from thence some living seaweed and placed it in the water, which derived additional benefit from this vegetation, just as Dr. N. B. Ward found his fresh water had benefited by the plants he introduced. It is more than probable, however, that in both these instances the really beneficial vegetation was not that which was thus visibly introduced, but was the minute kind which grew parasitically on the plants and upon the inside of the vessels. Yet it must be admitted that this gentleman and this lady are the two first known persons who, keeping a chemical law in view, deliberately and purposely set about attaining means for its fulfilment in an aquarium.

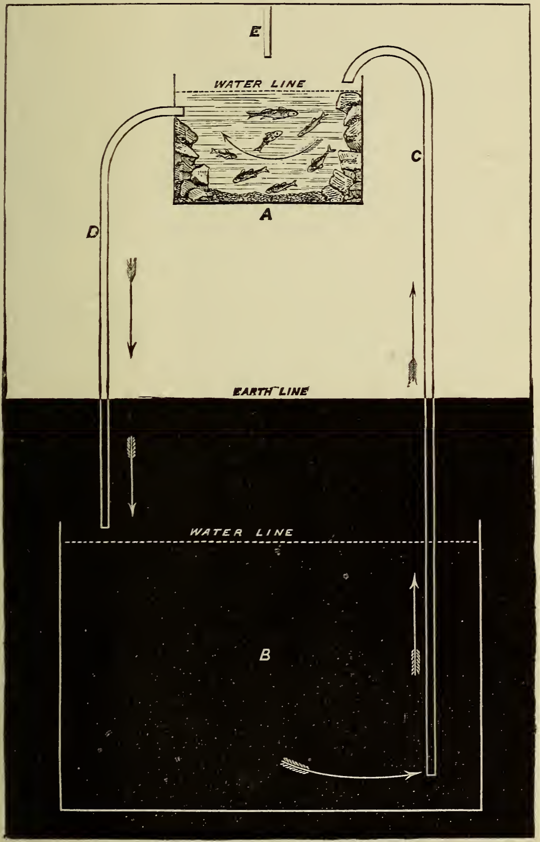
In 1849 the late Mr. Robert Warington, chemist to the Company of Apothecaries, set up in his rooms, in the Hall of that Company, in London, his first aquarium, a fresh-water one, followed, in 1851–2, by his first marine aquarium. These he described in the periodicals of the day, and in a lecture which he also gave at the Royal Institution in an interesting manner, and naturally from a chemist's point of view. At about the same period Mr. P. H. Gosse commenced his earliest marine aquarium, as did Dr. J. S. Bowerbank, Dr. Cotton, and the late Dr. E. Lankester, and the successes attained by these experimenters induced the Zoological Society of London to determine to have a public aquarium in its gardens in Regent's Park. The building for this purpose was erected in the spring and summer of the year 1852. The marine and fresh-water animals were begun to be introduced in the late autumn; the following winter and spring were wisely spent in experimenting on the best modes of operating, and the exhibition was opened on May 21, 1853. After having been noticed in print by the "Athenæum" of some months earlier, it was again commented upon by that journal of May 28, and by the "Illustrated London News" of the same day and year, the latter publication giving views of two tanks. One of the earliest services which this institution conferred on biological literature may be seen in portions of the Natural History division of the "English Cyclopædia" (an adaptation of the earlier "Penny Cyclopædia"), as the former publication appeared fortnightly, commencing in the spring of 1853; and as it was edited by Dr. E. Lankester, who always took much interest in aquaria, he mentions in the book from time to time that such and such animals named had been kept in this Regent's Park aquarium, to which he gave the needlessly long name of "Aquavivarium." This place was my own much loved and earliest place of Natural History studies, and in August 1853, I, too, arranged a little domestic aquarium of my own—a fresh-water one. Later, in the same year, I set up a small marine one, or rather a series of little aquaria in glass jars, holding from half-a-pint to a pint each. Seldom has a student begun with such very small means as I then possessed, for my sea-water was compounded of salts purchased at a London chemist's shop, and my animals were such little sea-anemones as I could find uninjured on oyster shells thrown into London streets. I was in earnest, however, and the difficulties I was so closely beset with, and they alone, enabled me to gain subsequent success. In the earlier books on aquaria—notably in Mr. Gosse's two volumes, his "Devonshire Coast" and his "Aquarium" (the latter having gone through two editions, 1853 and 1856, besides a recent reprint without the plates, which have been accidentally destroyed)—aquaria are associated in idea with conservatories, especially as to the growth of plants in each. This notion was very natural. Accordingly the Regent's Park aquarium was made virtually as a conservatory. But it was a diametrically wrong notion, as the first summer proved; and the second summer (1854) showed this still more conclusively; and the third (1855) yet more so, the evil being an accumulating one. It was then remembered, when too late, that marine and fresh-water plants and animals live in seas and rivers, where the temperature is much more restricted in range than that which obtains in the atmosphere.

It was seen that success was to be attained by representing these conditions of nature just named, and that to place such organisms in a glass house, where the rays of a summer's sun heated a mass of imprisoned air, was to kill the animals and to stimulate the plants to unnatural growth, or rather to cause them and some of the animals to be covered with a parasitic growth of the lower green algæ which obscured them. The errors of this earliest aquarium were strikingly shown by its solitary merit, the latter being its fresh-water division, occupying one side of the building, where the water coursed through the tanks in a constant stream, it being clear and cool, and peopled with an adequate number of healthy animals; while on the other side of the building, and in its centre, were the marine tanks, in which the water was, and still is, turbid and warm, and sparsely inhabited by not healthy creatures.

These good results were, however, obtained by accident and not design. The society possessed already a steam-engine, which pumped up water for the general use of its gardens, and it was a mere matter of course to connect the aquarium with this engine, and allow the water (which *chanced* to be drawn from a pure source) to run through the fish tanks, and then be applied to ordinary purposes, drinking or other, for which its passage through the tanks in no way unfitted it. I reasoned with the society that if the sea-water tanks were similarly treated on some such system as the fresh-water series, a correspondingly good result would be attained; and I pointed out that the same law governed both, because in the centre of the building were some isolated fresh-water tanks having no stream in them, and these were in a similarly ill condition as the marine tanks by their side. In reply, the society answered that a circulatory system did exist in a part of the sea-water series, but that it was almost useless; and I then pointed out that that was because the reservoir into which the sea-water entered after it had run through the show-tanks was too small in relation to the dimensions of the latter, and that the reservoir should be several times greater than the show-tanks. My reasoning was all in vain, however, for the society went on throwing away the sea-water when it was only *temporarily* unfitted for use, and getting at a cost of several hundreds of pounds yearly a weekly supply from the sea, especially when soon afterwards another evil made its appearance, consisting of a greenish-brown dense opacity permeating the water, and quite hiding from view all it contained. This was caused by excess of light, for I found that darkness removed it and made the water clear again; and this led to Mr. E. Edwards's invention of the dark-chambered tank, a modification of which is now, or should be, employed in all public aquaria where adequate results are aimed at and attained. So, at this early period, 1853-62, though in theory the Zoological Society of London, and everyone else who maintained aquaria, used the same unchanged water, especially sea-water, yet most persons sent to the sea, or to dealers, of which I was then one, for occasional new supplies. However, from 1853 to 1855, when I could not possibly get new sea-water for my little jars, I merely increased the quantity of water to about eight or ten times as much as those jars collectively held. Thus the aggregate contents of my jars were about six or eight pints; and in a now historical earthenware foot-pan, kept dark in a cool corner at hand, I had five or six gallons more water, containing neither animals nor plants, and when aught occurred to disturb the equilibrium of life in these jars, either from excess of light or heat by standing on a light window-sill, or from excess of food, or from there being too many animals in a small space, instead of throwing away the water thus *temporarily* rendered unfit to sustain life, I merely restored it to a right condition by pouring the contents of these jars into the foot-pan, which was so large in relation to the dimensions of the jars, that I could immediately dip them up full from it (the foot-pan) without the water being perceptibly the worse for it, especially when I so contrived matters that these transfers were made, not in one day, but on successive days. Thus, in London, far from the sea, which I had never seen, I was, so far, aquarially speaking, as well off as the wealthy Sir John Graham Dalyell, with the ocean almost at his door. Later on, in 1857–8, I set up another marine aquarium, in which the show-tank held 20 gallons, and the reservoir 500 gallons, of water, in which that water, instead of being intermittently circulating, as in my jar and foot-pan arrangement, circulated constantly, day and night, by means of a pump and pipes, in a cool underground London cellar or kitchen, with a uniform temperature of about 60° F. This answered excellently, especially when I increased the water in the reservoir to 1,000 gallons.

In 1860 I arranged in the Paris Acclimatation Gardens an aquarium which had been incipiently planned, or rather contemplated, by Mr. D. W. Mitchell, who had died some months before then, and I made the circulation a constant one, and gave as large an underground reservoir as funds would allow, but which was insufficient. In 1862 I went to Hamburg, where, with the aid of the late Mr. A. Lienau, an engineer of great knowledge, who saw the advantage of a large reservoir, I made the aquarium in the Zoological Gardens there, which was opened in 1864; and it was under my management so successful that it called other continental aquaria into existence, but not with so great a success, because of neglect in having the machinery so good, and the reservoirs so large, as they should be. But commercial companies, anxious for money success, and for that only, frequently fail from inattention to proper construction, and especially to hidden constructions which the public never see. In 1870 I returned to England, and, with Mr. C. H. Driver, arranged the Crystal Palace aquarium, with further improvements in machinery, and a still relatively greater reservoir. This, too, has been and is so very successful that I have been called upon to supervise the construction of several other public aquaria in Britain and abroad; and to perpetuate my modes of operation, both in construction and management, I now take pupils, who, when called upon, are ready to undertake the curatorship of aquaria in a scientific manner.

On March 1 last, Mr. W. S. Kent delivered a lecture in the rooms of the Society of Arts, the chief aim of which was to show that not large but (relatively to show-tanks) small reservoirs are necessary, or even no reservoirs at all. This is printed in the "Journal of the Society of Arts" of March 3 last, and my unanswerable reply to it may be seen in the same journal of March 24 last. If it be urged that small reservoirs may be made to do as makeshifts, because money and space for them cannot be afforded, there is some kind



of reason in *that*. But if it be averred to the contrary *as a principle*, then that indicates a singular amount of no knowledge which, if possible, is something more than wonderful. My arguments are founded on the clear and simple obviousness of the fact that a given quantity of dead organic matter diffused through a large quantity of water sullies it less than if it were small, and on the necessity of maintaining an evenly moderate temperature for the reasons already given, avoiding the high and low ranges of the atmosphere; and I show that the easiest manner of attaining this is by having a large reservoir sunk in the earth at a distance giving a known temperature. Thus, referring to the sunk thermometers at the Greenwich Observatory, with a thermometer having its bulb on a level with the scales of the sunk instruments, the lowest (January) mean monthly reading in a named year was 36·4° F., with a mean daily range of 6·9° F.; and under the same circumstances the highest (July) mean monthly reading was 66·9° F., with a mean daily range of 19·9° F. But from the showing of other thermometers whose bulbs are sunk in the ground to the respective depths of one inch, three feet, twelve feet, and twenty-five feet, the temperatures become strikingly even for the whole year through—so much so, that at twenty-five feet deep the mean monthly reading of January was 52° F., with a mean daily range of only 0·025° F.; and the mean monthly reading of July was 49·0° F., with a mean daily range of but 0·06° F., the highest mean daily range at that depth in any month of the year being 0·07° F. in August. To apply this to aquaria, I have made the accompanying diagram (p. 261), it being an ideal vertical section of an aquarium, consisting of a show-tank A, with its reservoir B in the earth, with an inlet supply-pipe C, and an outlet pipe D, the six arrows showing the direction of flow of water. E is a pipe supplying water to compensate for evaporation, which, both for marine and fresh-water aquaria, should be distilled water. For simplicity, the engines, pumps, &c., which circulate the water are omitted, the showing of results being alone aimed at.

Supposing that in any part of an English year the temperature of B would be 60° F., and that in summer A would rise to 75° F., that would be much too warm for an aquarium containing British animals. Or it might in winter sink to 30° F. or less, that would be much too cold. But on a sufficient circulation being established between A and B, then their mean temperatures would be expressed by the seven following formulas, varying according to the size of B:—

Formula No.	1.	A 2	. B 1	. Mean	result	70° F.
„	„ 2.	A 1	. B 1	. „	„	67·5° F.
„	„ 3.	A 1	. B 2	. „	„	65° F.
„	„ 4.	A 1	. B 3	. „	„	63·7° F.

Formula No. 5.	A 1	. B 4	. Mean result	63° F.
„ „ 6.	A 1	. B 5	. „ „	62·5° F.
„ „ 7.	A 1	. B 20	. „ „	60·7° F.*

Indeed if B were one hundred times as large as A, and were kept at 50° F., then A might be in an atmosphere at 212° F. (the heat of boiling water), and yet its water would be only 52·12° F., and the most delicate English animals would live in it. At Nottingham is an aquariam where the show-tank and reservoir spaces have had to be made as 13 is to 1. From Bünsen's tables in his "Gasometry," page 288, may be ascertained the amount of atmospheric air which water in open vessels will absorb at given temperatures, the barometer being at 39″; and I here reproduce his figures, having converted his Centigrade scale into Fahrenheit and Reaumur scale for the benefit of English, German, and Spanish readers.

Bulk of water = 1·

Temperature. C.	R.	F.	Air absorbed.	Temperature. C.	R.	F.	Air absorbed.
10 =	8 =	50 .	0·0195	16 =	12·8 =	60·8 .	0·0177
11 =	8·8 =	51·8 .	0·0192	17 =	13·6 =	62·6 .	0·0175
12 =	9·6 =	53·6 .	0·0188	18 =	14·4 =	64·4 .	0·0173
13 =	10·4 =	55·4 .	0·0185	19 =	15·2 =	66·8 .	0·0172
14 =	11·2 =	57·2 .	0·0182	20 =	16· =	68 .	0·0170
15 =	12 =	59 .	0·0180				

And therefore as the more air there is in the water the better it is, hence the value of large and therefore cool reservoirs. Independently of all this, however, the larger the bulk of water, and the more constant and vigorous the circulation and aeration, the less it will be sullied by the animals which live in it. In the Crystal Palace Aquarium we have in the show-tanks 20,000 gallons of sea-water, and in the reservoir 100,000 gallons, total 120,000 gallons, supplied by Mr. W. Hudson in 1870. Yet in this comparatively small quantity of unchanged fluid we have, from Sept. 1871 to March 31, 1876 (four and a half years), given to the animals in it the following enormous quantity of food without the water being otherwise than always sparklingly clear :—

* The water in the Crystal Palace aquarium has a very small range of from 52° F. in very cold, to 61° F. in very hot, weather. In April last (1876) we had, at Sydenham, blue skies, a bright sun, and an oppressive warmth, with 74° F. in the shade, on the 8th of the month. On the 12th, four days after, we had a leaden firmament, and clouds of blinding snow and sleet driven by a bitter north-east wind, with the thermometer at 29° F., giving so great a range as 45° F. within a week. Yet the water in the aquarium had a range of only 1° F. = 54° F. to 53° F.

1.	Sandhoppers (*Talitrus*), in pounds weight		12
2.	Shrimps (*Crangon*), in quarts		4735
3.	Crabs (*Carcinus*)	in gallons	137
	„ (*Cancer*), large	„ numbers	1450
4.	Scallops (*Pecten*) large, in numbers		32
5.	Oysters (*Ostrea*) „ „		2195
6.	Cockles (*Cardium*), in gallons		18
7.	Mussels (*Mytilus*) „		3544
8.	Whelks (*Buccinum*)	in gallons	7
		„ numbers	100
9.	Fish, chiefly Whiting (*Gadus*), in pounds weight		3159
10.	Smelts' roe (*Osmerus*) „ „		14
11.	Green seaweed (*Ulva*), purchased „		400
12.	„ „ (*Conferva*), grown in tanks, quantity unknown.		

And, in addition, we obtain occasional and unrecorded supplies from neighbouring fishmongers when the regular supply runs short. Of this animal food, all but the denominations 9 and 10 are kept alive in a series of reserve tanks till the moment of being eaten. Scarcely any uneaten food, and never any excrement, is manually removed; but all which is not consumed by the animals is chemically dissipated, without filtering, by the enormous volumes of air constantly being injected into every tank by Leete Edwards and Norman's machinery, the speed of which is accelerated (*i.e.* the oxygenation is quickened) when the water is slightly turbid from an excess of organic matter. All this I have explained more at length in the "Official Handbook to the Crystal Palace Aquarium," and in "Observations on Public Aquaria," both published at the Crystal Palace. It is this power of oxygenating, or consuming, or burning, at a low temperature, termed by Baron Liebig "eremacausis,"* which expresses the real work done in an aquarium, and the force necessary to do that work. Even our thick beds of sand and shingle at the bottoms of each tank are so fully charged with air, that one thrust of a stick will release a pint of it in bubbles. This is a source of purification and health quite unknown till recently. Consequently the floors of our tanks (excepting the sea anemone tanks) are as speckless and as free from the blackness caused by sulphuretted and carburetted hydrogen gas, as on the day they were laid down in 1870. If we have an excessive growth of seaweeds anywhere, we turn in a shoal of grey mullet (*Mugil capito*), who nibble it down close, like sheep in a field of grass. This leads me to say that at present we do not know how to grow the higher marine algæ, the red, the brown, or even the

* From the Greek "to remove by burning, or by fire." The words "caustic" and "cautery" have the same derivation.

green kinds, at will. Sometimes I succeed, but always by chance, not knowing why.

Of the general influence of aquaria on Zoology we have curious evidence in Mr. Gosse's most excellent "Manual of Marine Zoology for the British Isles," published in two volumes, in 1855–1856, in which the author enumerates 1,785 species, from sponges to fishes, and of which he figures 779 genera, always preferring to draw from living animals whenever possible. Now, as at that period a larger number of aquarium animals had passed through his hands than through those of any other person, he may be presumed to have, up to then, seen more of them alive than anyone else. Yet he enumerates only 201 as having been drawn from life, as he avowedly preferred doing, and of these but a dozen were fishes, others being, for the most part, small creatures, or those which are easily maintained, and do not need large tanks and elaborate machinery. But, during the twenty years which have elapsed since 1856, I have seen and handled, and had under my care, in England, France, and Germany, about 433 species of British marine animals, of which 112 were fishes.

There are few things more trying to that great virtue—patience—than a large public aquarium, especially in its preparation, before it is ready for the reception of animals. It is to this lack of patience on the part of the directors of the Royal Westminster Aquarium, and to their absolute refusal to allow me to have proper engineering assistance during its construction, and to general mismanagement, that its present confused state, and its unsatisfactory condition in every way, is due. On this account, I resigned my post of adviser to the society, as I found it useless to advise when advice was recklessly disregarded. Aquarium work, being hydraulic engineering on a small scale, is essentially the work of an engineer, and not that of an architect, unless he is also an engineer and a mathematician. There is for aquaria a great and important future, both as regards their influence on science, and as pecuniary speculations, if indeed, as I much doubt, there can be any real severing of these two interests. Success, however, must always be the result of a careful study and representation of what nature does, and of a strict avoidance of the recent heresies to which I have in this communication adverted.

POPOFFKAS, OR CIRCULAR IRONCLADS.

By A. HILLIARD ATTERIDGE.

SPEAKING at Nicholaiev in May 1875, before an audience chiefly composed of officers of the imperial navy, Admiral Popoff briefly but clearly set forth the principles which have guided the Russian Government in the reorganisation of its navy since the Crimean War. The resources of Russia, he said, did not permit her to undertake the construction of an offensive navy—a fleet of huge line-of-battle ships. It was therefore decided that the object to be held in view should be the formation of a strong defensive fleet, so powerful as to be able to protect the coasts of Russia from any possible coalition, and in 1871 he submitted to the Admiralty at St. Petersburg designs for what he suggested should be the typical form for the ships constructed for the coast defence of the empire. Admiral Krabbé approved of the project; the ship was laid down at Nicholaiev, the great naval arsenal of Russia on the Black Sea; and three years after the *Novgorod*, the first circular ironclad, was launched upon the Boug; and a few months later, fully armed and equipped, steamed down the river to the Black Sea, and made a successful voyage to Sebastopol. A second ship, originally called the *Kiev*, but named on launching the *Admiral Popoff*, in honour of her inventor, was begun and completed shortly after the *Novgorod*; and these two circular ships now form the principal strength of the Black Sea fleet.

Mr. Reed, the late chief constructor of the British navy, from the first urged the consideration of the structure of these new vessels upon public attention in England. They were avowedly a further development of the principle which he had adopted in shortening the length and broadening the beam in the later ironclads which he designed for our navy. He saw the keels of the *Novgorod* laid at Nicholaiev; in "Naval Science" he devoted several articles to the new ships, and published the first drawings of them which appeared in England. Last autumn he went to the Black Sea, made a voyage in one of the

Popoffkas, and described her performances in the remarkable letters to the "Times," which at length attracted general attention to the subject; and then, on Feb. 4, he gave a full account of the circular ironclads in a paper read before the Royal United Service Institution, in which he spoke of the introduction of this new type of man-of-war into a European navy as marking the beginning of a revolution in naval architecture. The Popoffka was originally designed only for coast defence; but Mr. Reed would have us make such vessels, with or without certain modifications, the type of the line-of-battle ship of the future. We propose to review the subject here; but, as the Popoffkas have been so fully and so recently described, we need not enter into the details of their construction at any great length.

The Popoffka is, roughly speaking, a saucer-shaped ship. With the exception of a slight projection at the stern, the ship is in outline a perfect circle. The deck is about 18 inches above the water, but curves upwards towards the centre to a height of about 4 feet. The bottom is flat, with several parallel keels, but its diameter is much less than that of the deck, and the side curves upwards, any section of it being a quadrant of a circle whose radius is equal to the depth of the ship. In the centre of the deck is what has been called a fixed turret, but it is really an open breastwork over which two heavy guns fire *en barbette*. The deck, the breastwork, and the sides of the ship, are all armoured. In front of the breastwork, at what is called the bow of the vessel, there is a lightly-built forecastle, and before this hang the anchors, the cables passing into it through hawse-holes, and thence to the chain-lockers. The two funnels are placed amidships, one on each side of the deck. There are six engines, each driving a screw, the six screws being arranged three on each side of the rudder along the sternward part of the circumference, the screws working independently, and the shafts being laid parallel to each other and to the line from bow to stern. On each side of the diameter there are two boiler-rooms each containing four boilers. The engine and boiler-rooms and coal-bunkers occupy the sternward half of the ship. In the middle are the powder-magazines and the shot-and shell-rooms, and in the forepart are the chain-lockers, water-tanks, and store-rooms. Above these and in the forecastle are rooms for the crew, the officers' rooms being placed in the centre of the ship under the breastwork. A passage runs round the ship, another traverses her longitudinally, and these form the chief means of communication under the upper deck. The armour on the sides of the *Novgorod* is from 9 to 11 inches thick, including an allowance for the hollow iron girders filled with teak which form the backing. The armour of the breastwork is 11 inches thick. The thickness of the side-armour of the *Admiral Popoff* is from 16 to 18 inches, the breastwork being protected with 18-inch armour. The armour is carried to a depth of $4\frac{1}{2}$ feet below the water-line. In both vessels the curving deck is covered with armour $2\frac{3}{4}$ inches thick, this thickness being considered quite sufficient to resist shot which must necessarily strike at low angles, as the Popoffkas are not intended for the attack of fortresses. The two guns in the breastwork are mounted on a platform which revolves on a central upright axis; this axis is hollow, and in action the charges for the service of the guns are passed up through it. The recoil is regulated and stopped partly by hydraulic compressors, partly by wedges in the back part of the slides. In action or in stormy weather all apertures are closed except those within the breastwork. Through these the ventilation of the ship is kept up, the air currents being formed by revolving fans. A light rail runs round the deck, and there is a bridge aft of the breastwork and level with the top of it. The engine-room hatchway is under this bridge, and the combings are carried up to it, so that it is safe from being flooded by the sea, which, from the extremely low freeboard, must frequently break over the deck.

The principal dimensions of the two ships are as follows:—

	Novgorod		*Admiral Popoff*	
	Ft.	Ins.	Ft.	Ins.
Extreme diameter	101	0	121	0
Diameter of flat bottom	76	0	96	0
Depth of hold	13	9	14	0
Draught of water, forward	13	2	12	0
Draught of water, aft	13	2	14	0
Height of top of breastwork above the water-line	12	0	13	3
External diameter of breastwork	30	0	34	0
Displacement	2,490 tons.		3,550 tons.	

The nominal aggregate horse-power of the six engines is in the *Novgorod* 480, in the *Admiral Popoff* 640. But the actual indicated horse-power is very high, that of the *Novgorod* at full speed being 2,270. The capacity of the coal-bunkers is very small; the *Novgorod* can carry only 200 tons, the *Admiral Popoff* only 250. The former vessel has a crew of 110, the latter of 120 officers and men.

The highest speed ever attained by the *Novgorod* was $8\frac{1}{2}$ knots, with an indicated horse-power of 2,270. Her ordinary speed is about $6\frac{1}{2}$. These are important figures, for it is on these data that the whole question of the efficiency or non-efficiency of the Popoffka as a ship of war must depend. We shall examine this question from two points of view—first, what is the value of the Popoffka as a vessel intended for coast defence in the shallow waters of the Black Sea, or the Sea of Azov; and secondly, is the Popoffka system applicable to vessels intended for ordinary warlike purposes and for naval operations in the open sea?

Now it must be quite evident that the *Novgorod* and the *Admiral Popoff* are not ships at all, but only floating batteries of novel construction. Even the maximum speed of $8\frac{1}{2}$ knots obtained by the *Novgorod* on a special occasion is a low one; lower still is her ordinary speed of $6\frac{1}{2}$ knots. Their coal supply is so small that they cannot cruise, but must simply use it to reach some station selected for defence, and then to work their ventilating fans and gun-platforms. The low circular hull is little better than a movable pontoon floating the breastwork and the guns. The Popoffka, if she sought an action, could not overtake the slowest ironclad in any European navy; or if she wished to avoid one, she could not escape. A hostile ship could choose her own position and distance in fighting her; and if an enemy chose to use his ram, the *Popoffka*, with her inferior speed, would find great difficulty in avoiding it, let her work her six screws as she would. The open turret or circular breastwork offers very little protection to the men working the guns. The two 28-ton guns of the *Novgorod* are mounted so as to fire *en barbette*—that is, over a breastwork which has no portholes—so that the men are continually more or less exposed to fire. The 40-ton guns of the *Admiral Popoff* are in a better position, for they are mounted on the disappearing principle; but in both ships the crews of the guns would be exposed to bursting shells; and in addition to this, in the *Novgorod* the men at the guns would suffer from showers of shrapnel-balls at long ranges and rifle fire at close quarters. Again, there is no overhang at the stern to protect the six screws. In fighting a single antagonist or defending a narrow channel, the *Popoffka* would, of course, protect her screws by keeping her bow to the enemy; but if she had to engage more than one hostile vessel, her whole motive power might be destroyed by a few well-directed shots.

It is easy enough to say that an overhang could be added to protect the screws, and that a closed turret could be substituted for the breastwork, but the mere fact that this has not been done by Admiral Popoff is enough to show that serious difficulties stood in the way of such an arrangement. And it is quite evident in what these difficulties consist. The ships have been built to carry thick armour and heavy guns on a light draught and small displacement. For this the circular form was adopted, and to this every other consideration was sacrificed.

Now the addition of the overhang and extra weights in the turret would have to be compensated by a decrease in the weight of armour and guns, or there would be an increase in the draught of water. Moreover, the adoption of the closed turret would have to be attended with numerous serious modifications in various structures on the deck. At present, by adopting the *barbette* arrangement, the only obstacles to an all-round fire are the two funnels, one on each side ; and of course it is easy for the Popoffka to take such a position with respect to the object aimed at that the funnels are kept out of the line of fire. But if a closed turret were substituted for the breastwork, the guns would have to fire through portholes, and either the turret would have to be made much higher than the present breastwork, so as to have its ports 12 or 13 feet above the water, or the forecastle, the bridge, and the high combings of the engine-room hatchway, would have to be lowered or done away with altogether. Now this forecastle plays a very important part in the economy of the ship. It contains the chief quarters for the men, and it protects the upper deck from the immense bow-wave raised by the blunt circular bow as it is (so to say) forced violently through the water when the ship is under steam. "The forecastle," says Mr. Reed, "of course adds greatly to the buoyancy forward when the sea rises there upon the vessel, and I do not think even circular vessels of very low freeboard could be steamed against a heavy head-sea without such a forecastle, more especially, as we shall see hereafter, when driven at a high speed." The forecastle, then, and the other deckhouses, must be maintained ; so that if we are to remove the badly protected open breastwork, and substitute a closed turret for it, that turret must be of great height, adding largely to the weight carried by the vessel, increasing her draught of water, and thus taking away from her one of her few advantages.

We have marked out the want of speed as the chief defect of the Popoffka system, and necessarily closely connected with this is the small coal-carrying power. A round ship could indeed be driven through the water at a high speed, and so could a square one for that matter ; but this is not the point at all. The shape of a ship is, of course, one of the main elements in a question of speed ; the others are her draught of water and the power of her engines. On her shape chiefly depends the amount of resistance offered by the water. Leaving the question of draught of water out of account, resistance is least in a ship built on such lines that her horizontal section at the water line generally corresponds to the wave lines formed by the water as it is parted by the bow and unites again astern. In the case of a short ship with a broad beam, like many of our ironclads, when steaming at any high speed, for want of this correspondence of the lines of the ship and the natural course of the wave lines, the water is piled up round the bows, and forms the bow-wave which floods the forward portion of the decks of low freeboard ships like the *Devastation*. This bow-wave is a kind of visible index of the resistance of the water, and in the Popoffkas it increases to the enormous mass of water which in a rough sea rises on the ship and makes the high forecastle a positive necessity. The bow-wave is a matter of some importance viewed in this light, and Mr. Reed's joke about people being anxious that the water should not be knocked about by his shortened ships is little to the purpose; for the bow-wave simply means that the short-broad ship finds it more difficult to get through the water than her longer competitor, and the round Popoffka finds it most difficult of all. The circular shape, so to say, handicaps the engines; they must have a power out of all proportion to the speed obtained. To rise to a high rate of speed, the weight of engines, boilers, and fuel would have to be enormously increased, and the weight of guns and armour reduced in a corresponding degree, or else the draught of water would have to be augmented. And with the adoption of either alternative, the special object for which the Popoffkas were designed would have to be sacrificed. Little if anything would be gained by giving the ship a larger diameter, and thus increasing her floating power; for the greater the diameter the greater the resistance of the water, and consequently the more powerful and the heavier the engines that would be required. This increase of resistance with the increase of diameter is also the fatal objection to the proposal of a lightly-built outer ring to contain bunkers, which would supply the present deficiency in coal-carrying power.

This question of speed and engine-power is, we believe, the real test of the whole matter. Mr. Reed, of course, recognises the fact; and in his lecture or paper, read at the Royal United Service Institution, he endeavoured to prove that "the circular ironclads have started with a much less proportion of steam-power to citadel armour and guns than has usually been given to ironclads, and not with an enormously greater power." To our mind his facts and figures disprove his conclusion. He acknowledges that, "as compared with ordinary forms, the power required to drive at high speed a circular ship of equal displacement would probably be from two to three times that required for an equal displacement obtained on ordinary lines, or even more." But, he said, the displacement did not offer a true basis of comparison; such a basis was to be found in the proportion of steam power to the weight of guns and armour carried. In other words, in the proportion of propelling power to the fighting power of the vessel propelled. With this we readily agree. Let us now examine Mr. Reed's figures, as given in the report of his lecture. He first took weight of guns and armour of the *Novgorod*, and the *Warrior*, and the *Defence*, which he selected as types of ordinary ironclads, and with this weight he compared the indicated horse-power at full speed. We give his tabulated results, merely adding to them the speed obtained:—

	Novgorod	*Warrior*	*Defence*
Weight of armour and guns protected .	806 tons	1,100 tns.	700 tns.
Indicated horse-power at full speed .	2,270	5,470	2,500
Proportion of power to weight driven .	2·8 to 1	4·9 to 1	3·5 to 1
Speed obtained	$8\frac{1}{2}$ knots	14·35 knots	11·6 knots

A little later on he made a similar comparison with the *Prince Consort*, the figures being:—

	Novgorod	*Prince Consort*
Weight of armour and guns protected .	806 tons	1,080 tons
Indicated horse-power at full speed .	2,270	4,234
Proportion of power to weight driven .	2·8 to 1	3 9 to 1
Speed obtained	$8\frac{1}{2}$ knots	13 knots

Here there is no real comparison. Mr. Reed should have taken, not the indicated horse-power of the *Warrior*, *Defence*, and *Prince Consort*, at full speed (that is, at from 12 to 14 knots), but their indicated horse-power at $8\frac{1}{2}$ knots, the speed of the *Novgorod*. As everyone at all conversant with the subject knows, and as Mr. Reed himself has shown very clearly in his work on "Our Ironclad Ships," the indicated horse-power developed in obtaining the full speed of a ship is out of all proportion to that developed for a speed of even two knots lower, the indicated horse-power at 14 knots being often double that at 12 knots; a still greater difference would be apparent if we were to compare the indicated horse-power at rates of speed so different as $8\frac{1}{2}$ and 14 or even 12 knots. Bearing this principle in mind, it is evident, even from Mr. Reed's figures, that the Popoffkas have been provided with engines relatively far more powerful than those of our fleet; and the ships Mr. Reed has chosen for comparison are some of the oldest of our ironclads, for the *Warrior* was launched in 1860, the *Defence* in 1861, and the *Prince Consort* in 1862. Why not select for this purpose some more modern vessel, with improved engines and a really heavy armament? We shall do this here with one of Mr. Reed's own ironclads, and following his method of investigation, but taking not only the full speed, but a lower speed also of the English ship; and we select for this purpose the *Hercules*, a ship designed by Mr. Reed, on the central battery system, and launched in 1870. She is armed with 18-ton guns, and protected with 9-inch armour.

Let us take first the *Hercules*, at her full speed of between 14 and 15 knots, and compare her with the *Novgorod* :—

	Novgorod	*Hercules*
Weight of armour and guns protected .	806 tons	1,534 tons
Indicated horse-power at full speed . .	2,270	8,529
Proportion of power to weight driven .	2·8 to 1	5·5 to 1
Full speed obtained	$8\frac{1}{2}$ knots	14·691 knots

Here, if we were to leave the difference of speed out of account, it would seem that the proportionate engine-power of the *Novgorod* is only half that which is given to the *Hercules*, whereas really the proportion lies the other way, and the Popoffka has to employ engines relatively more powerful than those of the *Hercules* to obtain a much lower speed. This will be clear from a comparison with the *Hercules* going at a little more than 12 knots an hour, the first real comparison that we have had yet, for here is something like equality in at least one of the elements :—

	Novgorod	*Hercules*
Weight of armour and guns protected .	806 tons	1,534 tons
Indicated horse-power	2,270	4,045
Proportion of power to weight driven .	2·8 to 1	2·6 to 1
Speed obtained	$8\frac{1}{2}$ knots	12·123 knots

So that with a lower proportion of engine-power the *Hercules* obtains a speed more than $3\frac{1}{2}$ knots higher than the full speed of the *Novgorod*. This, we think, clearly shows the fallacy of Mr. Reed's argument, drawn from the indicated horse-power at full speed of three of our oldest ironclads. We believe that these statistics of the performances of the engines of the *Hercules* at 12 and $14\frac{1}{2}$ knots as compared with those of the *Novgorod* at $8\frac{1}{2}$ knots, prove incontestably "that the circular ironclads have started with a much" *greater* "proportion of steam-power to citadel armour and guns than has usually been given to ironclads," the point which Mr. Reed endeavoured to disprove at the Royal United Service Institution on Feb. 4.

We arrive, then, at these conclusions. The Popoffkas, on account of their low rate of speed and small coal-carrying power, are necessarily adapted only for coast defence, and operations within very restricted distances from a port of departure. They can neither escape from, overtake, nor manœuvre against an enemy who has a high speed at his disposal. They have engines much more powerful than those given to ships built in the ordinary way, and a high speed cannot be given to them without an enormous increase of engine-power, which would necessitate a deeper draught or a lighter armament. If the ship is made larger to make room for these heavy engines and the requisite boilers and coal-bunkers, there will necessarily be a corresponding increase in the difficulty of forcing the great curved bow through the water. The screws are unprotected, the men working the guns in the open breastwork are very imperfectly sheltered, and the structure of the Popoffka makes it difficult to substitute a closed turret. The ship, then, cannot cruise; and even for coast defence she is a very imperfect engine of war. For the latter purpose gunboats of the *Staunch* class would be equally, if not more, efficient. These gunboats are small, low, unarmoured vessels, carrying one gun in the bow. Being fought bow-on they present a very small mark to an enemy; and if the gun were mounted on the Moncrieff principle, the gunners would be at least as well protected as those who will man the open breastwork of the *Novgorod.* There would be no difficulty in mounting and fighting a gun of 35 tons or even more in a gunboat of the *Staunch* class; the 18-ton gun is the heaviest yet mounted in these gunboats in our navy; but Messrs. Armstrong are, we believe, now constructing for a foreign government one which will carry a 25-ton gun. These gunboats have twin-screws, the gun is pointed by means of the helm, and with a bow-rudder the gunboat can reverse her engines and run astern fighting her gun, and still keeping her propelling power protected by being turned away from the enemy. Additional protection for the engines could be secured by the use of one or more armoured bulkheads. We have thus already in our navy a good type of a small light-draught vessel for coast defence, and we do not see what further advantage is to be obtained by the adoption of the Popoffka system.

As for Mr. Reed's suggestion that we should adopt the Popoffka as a central citadel for a cruiser, with a lightly-built bow and stern added to it, it must be considered on quite a different basis. The system of an armoured central citadel with unarmoured ends has already been adopted in our navy, and very highly developed in the plan of the *Inflexible*, which we described in these pages in January last. There is no prohibitory reason why the central citadel should not be round instead of oblong, as in the *Hercules* and the *Inflexible*; but, for our part, we prefer the oblong form as affording more room and fitting more easily to the ordinary form of the ship. But such ships in any case would not be Popoffkas; and the real question with regard to them would be the old one of the relative merits of short-broad ships and ships of ordinary proportions—a question which we do not intend to discuss here. We have endeavoured to show that the Popoffka is practically a useless form in naval architecture. We have only touched upon a few points, but these are sufficient. Certainly they are the most important, and we believe that they are the crucial tests of the efficiency or non-efficiency of the system. The whole subject is being experimentally investigated by Mr. Froude on the part of the Admiralty; and when his report is published we expect that the public will be in possession of a judgment upon the Russian circular ironclads very different from Mr. Reed's.

ON THE EXTINCT ANIMALS OF NORTH AMERICA.

By Professor WILLIAM HENRY FLOWER, F.R.S.

[PLATE CXXXVIII.]

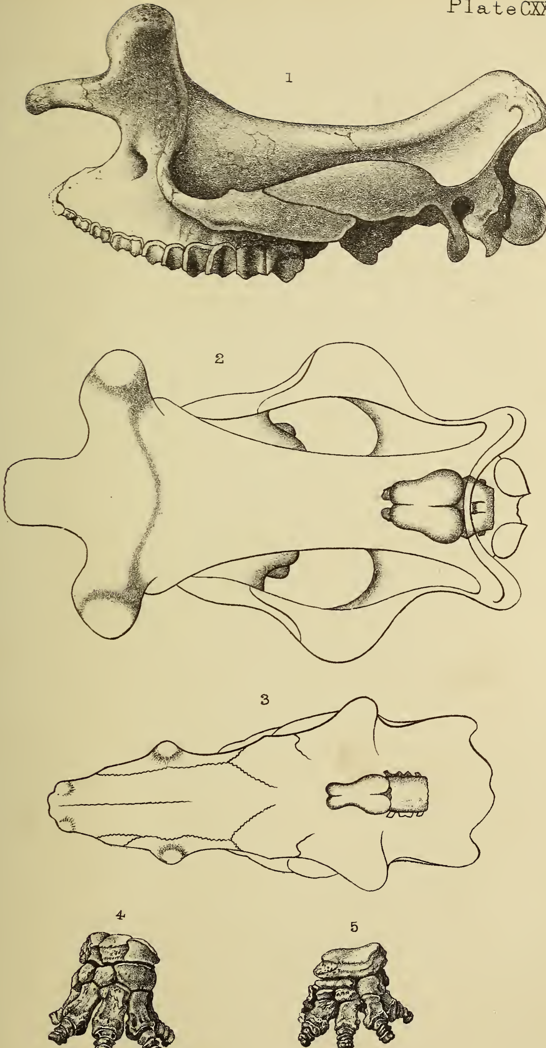
FEW branches of knowledge have received greater accessions during the last few years than that of the past history of the living creatures which have peopled the earth.

I propose in the present instance to call attention to some of the results that have been achieved, mostly within the last four or five years, by a small but energetic band of explorers upon a limited part of the earth's surface ; results the greatness of which already is only equalled by the promise they give of future still more important extensions of knowledge.

It is mainly through the agency of the admirably conducted geological and geographical surveys of the Western Territories, made by the United States Government, under the direction of Dr. F. V. Hayden, that the subjects to which I shall refer have been brought to light; surveys which are giving to the world, in an excellent series of publications, rich funds of information upon the physical geography, mineralogy, geology, palæontology, zoology, and botany of that hitherto little known but most remarkable region of the earth embracing and bordering the great range of the Rocky Mountains. For the special knowledge which we in England possess of the vertebrate fossils which have been discovered by these surveys, we are greatly indebted to the excellent descriptions of Professor Joseph Leidy of Philadelphia, who in two large and beautifully illustrated volumes* has given the results of his investigations upon them. More recently two other naturalists, Professor E. D. Cope of

* "Extinct Mammalian Fauna of Dakota and Nebraska, with a Synopsis of the Mammalian Remains of North America," 'Journ. Acad. Nat. Science,' Philadelphia, 1869; and "Contributions to the Extinct Vertebrate Fauna of the Western Territories," 'Report of the U. S. Geological Survey of the Territories,' Washington, 1873.

Plate CXXXVIII.



W. West & Co. lith.

American extinct Mammals.

Philadelphia, and Professor O. C. Marsh of Yale College, have taken the subject in hand, both as explorers and describers.*

It must be premised that the material has come to hand so rapidly during the last three or four years that most of the information which has hitherto been given to the world, especially by the two last-named palæontologists, is in a very provisional and fragmentary state; and that until the flood of new discovery begins to ebb, and the few labourers in this plentiful harvest-field have leisure to prepare careful, elaborate, and, above all, well-illustrated descriptions of the specimens, we shall remain in much uncertainty about the real nature and relations of many of the animals of that strange old fauna, which at present are to us little more than names.

I must presume that readers are familiar with the main epochs of time into which geologists have divided the earth's history. For the present purpose we shall only have to refer to the latest of these, the Tertiary, representing how many millions of years we cannot say; and which, for convenience, is generally subdivided into four sub-epochs, the Eocene, the Miocene, the Pliocene, and the Pleistocene, the end of which brings us to the period in which we now dwell. Of course it is not implied by this division that there was any sudden break or interruption of the steady course of the world's progress between these periods. They are merely artificial and arbitrary, though convenient stages, and pass insensibly one into the other; but without the use of some such terms we could not fix the epoch of any particular event or set of events. In geology we know nothing of centuries. We have no kings' reigns, as in political history, to mark the course of time, so we speak of "Miocene" much in the same vague kind of sense in which we speak of the "Middle Ages" in our chronology of the historical events in Europe.

The first evidence of mammalian remains in strata of Miocene age in Western America was that made known in 1846 by Dr. Hiram A. Prout, of teeth then supposed to belong to a gigantic species of *Palæotherium*, and subsequently described by Leidy under the name of *Titanotherium*. This was the commencement of that interesting series of discoveries, which have now made the "Mauvaises Terres," or "Bad Lands," of the White River of Dacota, classical ground to the palæontologist. But it was not until 1869 that the older beds on the western side of the Rocky Mountains were explored, and the more ancient Eocene land fauna of North America brought to light. In that

* I am glad to take the opportunity of thanking Professors Hayden, Leidy, Marsh, and Cope, for their kindness in sending me copies of all their numerous memoirs bearing upon the subject of this article.

year commenced the explorations in the vicinity of Fort Bridger, a military post situated in the south-west corner of Wyoming Territory, which have yielded such an abundant harvest, and the locality of which is thus graphically described by Professor Leidy:—

"Fort Bridger occupies a situation in the midst of a wide plain, at the base of the Uintah Mountains, and at an altitude of nearly seven thousand feet above the ocean level. The neighbouring country, extending from the Uintah and Wahsatch Mountains on the south and west to the Wind River Range on the north-east, at the close of the Cretaceous epoch, appears to have been occupied by a vast fresh-water lake. Abundance of evidence is found to prove that the region was then inhabited by animals as numerous and varied as those of any other fauna, recent or extinct, in other parts of the world. Then, too, a rich tropical vegetation covered the country, in strange contrast to its present almost lifeless and desert condition.

"The country appears to have undergone slow and gradual elevation; and the great Uintah Lake, as we may designate it, was emptied, apparently in successive portions, and after long intervals, until finally it was drained to the bottom. The ancient lake-deposits now form the basis of the country, and appear as extensive plains, which have been subjected to a great amount of erosion, resulting in the production of deep valleys and wide basins, traversed by Green River and its tributaries, which have their sources in the mountain boundaries. From the valley of the Green River the flat-topped hills rise in succession, as a series of broad table-lands or terraces, extending to the flanks of the surrounding mountains.

"The fossils which form the subjects of our communication for the most part were derived from the more superficial deposits of the great Uintah basin, which Professor Hayden has distinguished as the Bridger group of beds. These compose the terraces or table-lands in the neighbourhood of Fort Bridger, and consist of nearly horizontal strata of variously-coloured indurated clays and sandstones. As the beds wear, through atmospheric agencies, on the naked declivities of the flat-topped hills, the fossils become exposed to view, and tumble down to the base of the hills among the crumbling *débris* of the beds."

The immense length of time that this ancient lake has existed may be inferred from the fact that the mud or sand deposited in it has accumulated to more than a mile of vertical thickness.

It is from this and from neighbouring localities systematically explored only during the last four or five years, both by the Government surveys and by expeditions organised for the purpose from Yale College, that most of the remarkable animals attributed to the Eocene epoch have been obtained; although still more recently fossiliferous beds of the same age have been discovered both in Colorado and New Mexico, so rich as to give hopes that we are still only on the threshold of our knowledge of the wonderful fauna of the ancient American continent. Besides the extensive and older known Miocene and Pliocene beds between the Rocky Mountains and the Missouri, others of corresponding age have been discovered west of the Blue Mountains in Eastern Oregon.

I must now pass in successive review some of the principal groups into which animals have been divided by naturalists, and show what is known of their past history on the great North American continent. I am aware that the summary I am about to give will be exceedingly imperfect, partly on account of the limited time allowed in one discourse, and partly on account of the difficulty of extracting a connected account of these discoveries from the exceedingly numerous notices in which they are described—often fragmentary and disconnected, and even contradictory, and scattered through a variety of periodicals and reports. As most of these descriptions are put forth by their authors as "preliminary," to be superseded by more elaborate and detailed work hereafter, so must this notice of them be regarded. It will at least serve the purpose of calling attention to the importance and interest of this comparatively new field of research.

The first group to which I will direct attention, as it is that of the ancient history of which we have more complete knowledge than of any other, is the large order of *Ungulata*, or hoofed animals; and first among them I will consider those characterised, among many other distinctive peculiarities, by the uneven or *perissodactyle* structure of the foot, represented in the actual fauna of the world only by the three families of Horses, Tapirs, and Rhinoceroses—animals differing very considerably from each other in general outward appearance, and yet having many important common characteristics.

It is well known that in the Old World, species of this group, very intermediate in characters to those now existing, flourished in the Eocene age. Cuvier's grand researches in the Paris gypsum beds, which laid the foundation of the study of mammalian palæontology, reconstructed the form, now almost as well known as that of the existing tapir, of the *Palæotherium*; and numerous allied species have since been found, not only in France, Switzerland, and Germany, but in the corresponding beds in our own country. But in America, before 1869, not a single Eocene Perissodactyle had been discovered. In fact, as just mentioned, no Eocene beds containing the remains of terrestrial animals had been explored. Since that date, however, it has been ascertained that the region in which the stupendous mountain ranges of western North America have since been elevated were tenanted by animals of similar form and characters, and quite as varied in species and as numerous in individuals as those which at a corresponding period of time ranged through the marshes and forests of the Paris and London basins.

None of these appear to be identical specifically with the European forms; and even the generic indications, being often founded on very limited portions of the organisation only, as a few teeth, must be regarded as provisional. Many were undoubtedly quite distinct from any which we know from the Eastern world. It would be useless here to give a catalogue of the generic and specific names which have been given to the animals of this group already discovered. A brief mention of the most important and interesting will suffice. The two best known genera are those named by Leidy *Hyrachyus* and *Palæosyops*; the former is allied to the Lophiodons and Tapirs, the latter to the Palæotheriums. They both embrace animals in size varying from that of a small rhinoceros down to a peccary. The numerous modifications and combinations of characters found in forms apparently allied to them, which are little known to us at present except by the names given to them by their discoverer, will doubtless afford for a long time to come materials for the minutest scrutiny. Some appear to be allied to the European *Lophiodon* and *Hyracotherium*, one of which *Orohippus* (Marsh), seems to connect these forms through *Miohippus* and *Mesohippus* with the horse-like *Anchitherium*, and thus fill a link wanting in European formations in the pedigree of the Equine family. This animal, like so many other of the Eocene Perissodactyles, resembles the modern tapirs in retaining the fifth digit on the fore-foot, though, as in all known members of the group, the first was wanting, and both first and fifth were wanting to the hind-foot. Several species are described, but none larger than a common fox. One form only, *Diceratherium* (Marsh), is rhinocerotic. It is found in the uppermost Eocene strata of Utah, and gives the earliest indication of this group yet known. It seems, according to Marsh, to be connected with the lower Eocene *Hyrachyus* on the one hand, and the Miocene *Hyracodon* on the other.

In the Miocene period in North America the Perissodactyles attained a great development of form, variety, and size, the groups became more distinctly separated from each other, and some of them possessed remarkably specialised characters. True tapirs have not yet been met with in this period, which is rather remarkable, taken in conjunction with the present geographical distribution of that group. The Palæotheroid and Lophiodont forms had nearly, if not quite, died out, but the more horse-like *Mesohippus*, *Miohippus*, and *Anchitherium* were abundant, and appear to continue the line from the Eocene *Orohippus* to the true horses of the Pliocene period.

Rhinocerotic forms now became ascendent, being represented by *Diceratherium* (Marsh), differing from all existing animals of the group, by having a pair of horns placed side by side on the nasal bones; and a very interesting genus, *Hyracodon* (Leidy), an animal with molar teeth and many other of the characters of rhinoceros, but having no nasal horn, and having a complete set of incisor and canine teeth, as in all the older Perissodactyles, which are lost in the more modern rhinoceros. It is therefore quite a connecting link between the Palæotheroid animals of the Eocene, and the true rhinoceros of the Pliocene, and occupies exactly the right geological horizon that such a form ought to do if the one has been genetically derived from the other.

Hyracodon, therefore, has a high place of interest among many of similar nature which have been revealed by our newly-acquired knowledge of the ancient American fauna. If, however, as is stated, the fifth digit of the fore-foot is only rudimentary, it could scarcely have been, as remarked by Marsh, on the direct line of descent from the four-toed Eocene to the equally four-toed Miocene rhinoceros, though certainly in such a case we know not what ought to be allowed for reversion.

The same period (generally speaking) also produced several species of a more perfect rhinoceros, still hornless, however, and resembling the contemporaneous European *Aceratherium*.

But the most remarkable of the Miocene Perissodactyles, and in some respects the most remarkable of all the animals which the recent explorations have brought to light, are a number of species of gigantic size, to the first known of which Leidy gave the name of *Titanotherium*, and of which other forms have been named by Marsh *Brontotherium*, and by Cope *Symborodon*.

They must, by their great size and strength, grotesque appearance, and general mode of life, have taken the place in the Miocene times of the then extinct *Uintatherium* of the Eocene, and were in their turn replaced by the Mastodons and Elephants of later ages. The rhinoceros of the present day will serve to give the best general idea of the appearance of these creatures, but some of them (for they seem to have been numerous both in species and individuals) approached nearer to the elephant in size and length of limb. The skull (Pl. CXXXVIII. figs. 1 and 2) in its general characters was quite rhinocerotic; but the nasal bones supported a pair of large, laterally divergent, rugged prominences, apparently for the attachment of horns. The limbs were intermediate in proportions between those of the elephant and rhinoceros; but, as in the latter, the femur has a third trochanter, and a deep pit for the round ligament. The feet were short and stout, but in essential characters agree with the true Perissodactyles, and have four toes in front and three behind.

Numerous species have been described, both by Cope and Marsh, founded chiefly upon the form and direction of the horn cores on the nasal bones: they are all from the Miocene beds east of the Rocky Mountains, in Dakota, Nebraska, Wyoming, and Colorado; and there is no evidence of any of the *Titanotheridæ*, as the family should be called, after the first-characterised genus of the group, having survived to a later geological epoch.

Fig. 1.

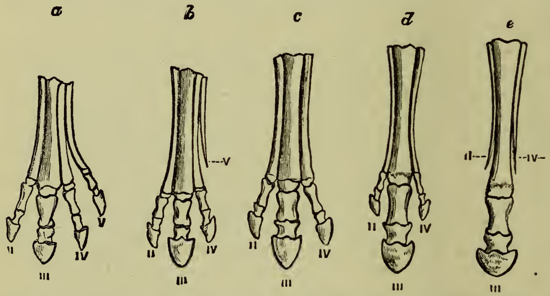


Diagram of several stages of modification of the feet of extinct forms of American horse-like animals (chiefly from Marsh), showing gradual reduction of the outer, and enlargement of the middle toe (III).

a, Orohippus (Eocene); *b, Mesohippus* (Miocene); *c, Miohippus* (Miocene); *d, Hipparion* or *Protohippus* (Pliocene); *e, Equus* (Pleistocene).

When we pass to the Pliocene and Pleistocene times, the Perissodactyles met with can all be referred to one or other of the three existing families; all the intermediate forms, and all those which have attained a different type of specialisation, as those last named, have completely disappeared.

Remains of several species of *Rhinocerotidæ* were very abundant during the Pliocene period in western North America; they all appear to belong to the hornless type, and from causes unknown became entirely extinct before the Pleistocene age. No rhinoceros exists now on the American continent, nor is there evidence that it ever supported animals belonging to the minor groups of the family to which the existing Indian, Sumatran, or African rhinoceros pertain.

During this period an immense development took place in the various forms of three-toed horses, *Anchippus*, *Protohippus*, *Parahippus*, *Hipparion*, &c., which replaced the *Anchitherium* of the Miocene. These in their turn, through many well-marked gradational forms (a full knowledge of which is among the many interesting results of these explorations), gave way to the true horses, of which remains of several species have been found in Pleistocene deposits, scattered throughout almost every region of the continent from Escholtz Bay in the north to Patagonia in the south. These also became entirely extinct before the discovery of America by the Spaniards—a most remarkable circumstance, considering the fitness of the country for their maintenance, as proved by the facility with which the descendants of horses introduced by European invaders have multiplied in a feral state.

On the other hand, of tapirs, fossil remains have been found most sparingly, though sufficient to show that they had a much wider geographical range northwards in the Pleistocene period than now, and yet several species of this genus still linger in the highlands of Central and South America, the sole direct representatives of the vast and varied Perissodactyle fauna of the continent in ages long gone by.

We may now pass to the remaining great group of hoofed animals, the even-toed or Artiodactyles, represented at present by the pigs, hippotami, camels, chevrotains, deer, antelopes, sheep, and oxen.

The remains of this group in the hitherto explored American Eocenes are very scanty and unsatisfactory as affording indications of its early history and development. Not a single specimen has yet been described which was found in a sufficiently perfect state of preservation to give a tolerably correct idea of its structure and affinities, and no forms corresponding to the well-established European *Anoplotherium*, *Dichodon*, *Xiphodon*, or *Cœnotherium* have been found. Towards the close of the period only (if the age of the Tertiary beds of Utah are rightly determined) do we find indications of well-defined crescentic-toothed or Selenodont species (*Agriochœrus*, Leidy), and also of tubercular-toothed or Bunodont forms (*Elotherium* and *Platygonus*). During the Miocene period, however, Artiodactyles of both these two main divisions abounded. It will be as well to take each group separately, and follow its history throughout, from the Miocene to the present day.

1. The Bunodonts, or those which inclined most to the pigs in dental structure. These were in North America, as in Europe, chiefly represented by animals of the genus *Elotherium*, huge swine-like creatures, some of whom approached the hippopotamus in size, and also by an allied four-toed form, *Pelonax*

(Cope), remarkable for its horn-like bony tubercles projecting out on each side from near the front end of the lower jaw. These became extinct, as in the Old World, before the close of the Miocene epoch.

Animals more like true pigs also existed, but all of the peccary type, the only one which now survives on the American continent. If the evidence of teeth alone can be trusted, this form, like the tapir (and the African *Hyomoschus*), is an unmodified remnant of the old Miocene fauna. But both at that period and in the Pleistocene, peccary-like animals existed in greater variety (as in the genus *Platygonus*), and in wider geographical distribution than at present. It is interesting to note that no remains of true *Sus* or any of its Old World modifications, as the wart hog (*Phacochœrus*), and babirussa, or of any species of hippopotamus, have hitherto been met with on the American continent. And thus the American Bunodont Artiodactyles, instead of undergoing great and diverse modifications, as did the corresponding animals of the Old World, have been gradually dwindling and contracting to the two closely-allied species of peccary (*Dicotyles tajacu* and *D. labiatus*), among the smallest and most insignificant of the whole group.

2. Of the Selenodont or crescentic-toothed Artiodactyles, the former existence in America of the long-extinct Old World genus *Hyopotamus* has been recognised by the discovery of a few teeth in the lowest Miocene of Dakota; and this is remarkable as the only recorded instance of an American form with three cusps on one of the lobes of the upper molars, a very common character among the European Miocene Artiodactyles.

Remains have also been found recently of various small ruminant-like animals, some not larger than a squirrel in size, to which the names *Leptomeryx*, *Hypisodus*, *Hypertragulus*, &c., have been applied. Whether these belonged to the family of Chevrotains or *Tragulidæ* (improperly called pigmy musk-deer), or whether, as appears more probable, they were not rather generalised or ancestral forms of the true Pecora or Ruminants, is difficult to determine from the present evidence.

Perhaps the most interesting of the American Miocene Artiodactyles, on account of their great abundance both in species and individuals, the full information which has been collected as to their structure, and their distinctness from any known forms from any other part of the world, is a family to which Professor Leidy applied the term *Oreodontidæ*. They played the part in the North American Miocene fauna which the deer do now in the same country, the antelopes in Africa, and the sheep in Central Asia. They were in nearly all points of structure intermediate between the ruminants and pigs, and (with many other Old World forms, however) completely break down the line of demarcation which our knowledge, when limited only to the existing fauna of the world, caused zoologists to draw between those two groups.

They appear to have survived throughout the whole of the Miocene period, commencing in the genus called *Agriochœrus* in the uppermost Eocene, and ending in the *Merychyus* of the early Pliocene; and it is of great interest to know that a gradual modification can be traced in the characters of the animals of the group, corresponding with their chronological position, from the earlier more generalised to the latest comparatively specialised forms, thus affording one of the most complete pieces of evidence that is known in favour of a progressive alteration of form, not only specific, but even of generic importance, through advancing ages.

Another group of great interest made its appearance in the Miocene of North America, and which, if the evidence of fragments can be trusted, did not become extinct, like the last, but, continuing to exist through the Pliocene and Pleistocene ages, is still represented on two distant parts of the earth by the three or four species of llama of South America, and the two species of camel of the Old World. The discovery of the early Miocene *Pöebrotherium* and of the Pliocene *Procamelus* and *Pliauchenia*—remains of which, and of Pleistocene *Aucheniæ* of great size, though not generically distinguishable from the modern llamas, are abundantly distributed over the North American continent—seem to show that that country was the original home of the singular family of *Camelidæ*, which was probably introduced by emigration in its perfect condition into the Old World, where none of the transitional forms from the more generalised ruminants, like those above mentioned, have been met with.

On the other hand, of the gigantic four-horned *Sivatherium* of the Himalayas, the equally large but hornless *Helladotherium* of the Grecian Miocene, or of the giraffes, no representatives have hitherto been found in America. And very little light has been shed upon the origin and distribution of the true ruminants by these researches, except in a negative manner. No deer have been found in the Miocene (at which epoch they were abundant in Europe); in the Pliocene but a single and poorly developed species; while in the Pleistocene, with the exception of one large species, called *Cervus Americanus*, they scarcely differ from those of the actual fauna. Of the hollow-horned ruminants, several species of bison, of *Ovibos* or musk ox, and a single *Ovis*, have been described, all from the Pleistocene, but not a single species of antelope. From these facts it may safely be inferred that the few existing and Pleistocene hollow-horned ruminants are immigrants of recent date from other lands; and it is probable that the deer have been similarly derived, though at a somewhat older period, which will account for their being more varied and wider spread in surface distribution, extending down almost to the southern extremity of the continent, while the hollow-horned ruminants are entirely confined to the north.

Scarcely any group to which the term "Order" is applied is so limited in the number of its existing species as that called, from one of the most striking external characteristics of the animals composing it, "*Proboscidea.*" The two species of elephant, that of Asia and Africa, the largest and in some respects the strangest of all land animals, are its sole representatives. Between these two animals and all others now existing there is a wide gap in numerous essential structural characters, so that really, in the world as we now see it, they have no near relatives.

But this was not always so. Leaving the existing condition of the earth's surface, and passing back to the last well-marked stage before our own, the Pleistocene period, we find abundant remains of elephants, imbedded in alluvial gravels, or secreted in the recesses of caves, into which they have been washed by streams or floods, or where, in many cases, they have been dragged in as food by hyænas or other predacious inhabitants of these subterranean dens.

We find these remains of elephantine animals extensively distributed over regions of the earth where no such creatures have existed within the memory of man. We find, moreover, that the elephants of the Pleistocene period, judging from their bones, and especially their teeth, do not in most cases exactly correspond in form or size to either of the existing kinds. We certainly find remains undistinguishable from those of the existing African elephant, in the north of Africa and southern parts of Europe; but the majority of these remains not only differ among themselves, but differ more or less from either the African or Indian species, and hence have had many different appellations bestowed upon them, as belonging to what are considered to be different species.

But not only in the Pleistocene period did elephants abound. Animals which must come within any definition which will include both *Elephas Indicus* and *Elephas Africanus* are also found in the European Pliocenes; and even earlier in Asia, the deposits of the Sivalik Hills, belonging to a transition between the Pliocene and Miocene age, are rich with the remains of elephants of varied form, in some cases presenting a considerable departure from our better-known types. Further back in time, however, we search in vain for true elephants. In the Miocene period, it is true, many kinds of huge Proboscideans roamed over the surface of the earth; but these differ so much from what we now call "elephants," that it becomes necessary to distinguish them by another name; and that of *Mastodon*, first applied by Cuvier, is generally adopted.

Mastodons however were, after all, very like elephants, only being distinguished by some peculiarities of the teeth; and by means of intermediate species the two forms pass so gradually into one another, that it is difficult to say, in the case of some species, with which group they ought most properly to be classed. One other form of animal, which can be referred to the order *Proboscidea*, is known in the Old World—the *Dinotherium*, a huge beast, the nature of which for a long time was very doubtful, having been grouped by some naturalists with the Manatees, by others even with the Marsupials. Its remains have been found, though comparatively rarely, in Miocene deposits in Germany, France, Greece, Asia Minor, and India.

Here our knowledge of the history of the order *Proboscidea*, as derived from palæontological researches in the Old World, ends. The *Dinotherium*, being in its teeth and some other respects slightly less specialised than the other forms, constitutes some kind of an approximation to the Ungulate animals. especially the tapirs; but the gap to be bridged over is very wide; and no remains referable to animals of the order, or any intermediate forms between this and other orders, have been found in Old World Eocene deposits.

Let us now turn to America. Neither at the present time nor within the memory of man have any Proboscidean animals existed within the length and breadth of the whole continent. But at one time—and that, geologically speaking, a very recent one—both true elephants and true mastodons abounded in North America, and the last-mentioned genus extended far into the southern portion of the continent. The elephant, the remains of which are most abundant throughout what are now the United States, differed but very slightly, if at all, from that which at the same period ranged throughout the northern portion of the Old World from the British Isles to Alaska. The commonest species of mastodon (*M. Americanus*, or *Ohioticus*, or *giganteus*) seems to have survived to a much later period than any of its European congeners, and even to have been the last extinct of all the American Proboscideans. Remains of other elephants and mastodons, though not differing in any remarkable degree from well-known European forms, have been found in Pleistocene and (at all events with respect to the last-named genus) in Pliocene deposits; but, as far as the evidence is at present before us, nowhere earlier.

So far, then, we find that elephants and mastodons, of types quite resembling those found in the Old World, but in less specific variety, appeared on the American continent at a later period than in the Old World (none having been found of undoubted Miocene age), and ultimately became completely extinct before the historic period. No animal corresponding to the *Dinotherium* has been found. We shall hardly, then, be prepared to look for primitive types of the race in earlier American formations.

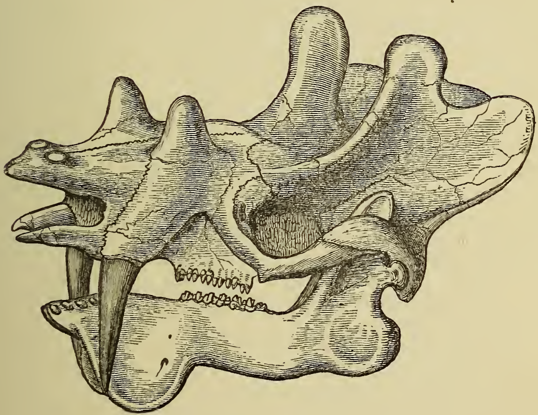
Among the most remarkable discoveries of the Eocene formations of Wyoming, has been that of a group of animals of huge size, approaching, if not equalling, that of the largest existing elephants, and presenting a combination of characters quite unlike those known among either recent or extinct creatures, and of which there were evidently several species living contemporaneously. Bones of some of these animals were discovered by Professor Marsh and Lieutenant Wann, of the Yale College exploring party, near Sage Creek, Western Wyoming, in September 1870, and described by the former in the following year, though provisionally referred to the genus *Titanotherium*. Other remains were discovered and described by Leidy in 1872, under the generic name of *Uintatherium* (from the Uintah Mountains, near which they were found). Very shortly afterwards other portions of bones and teeth of either the same or closely allied forms were described by Marsh as *Dinoceras*, and by Cope as *Loxolophodon* and *Eobasileus*. Whether these names will ultimately be retained for separate generic modifications, or whether they will have to be merged into the first, it would be premature to attempt to decide upon the evidence before us. Until satisfactory grounds have been shown for considering them to be distinct, it will be best to speak of them all under the name which has the priority.

To form some idea of the general appearance of one of these animals, we must imagine a creature very elephant-like in its general proportions, elevated on massive pillar-like limbs, not quite so long, certainly, as those of elephants, but with the femur vertically placed, without third trochanter, and without pit for the round ligament as in those animals, the radius and ulna complete, and crossing, and the feet short, broad, massive, and with five toes on each. At first sight the skeletons of these feet (as figured by Marsh) show an extraordinary resemblance to those of the elephant, and to no other animal (see Pl. CXXXVIII. figs. 4 and 5), especially in the form of the astragalus; but on closer inspection it is seen that in the mode of articulation between the different bones of the carpus and tarsus they really come nearer to the rhinoceros and other Perissodactyles. For example, the upper end of the third metacarpal, instead of joining almost alone to the magnum, as in elephants, is united by two nearly equal facets to the magnum and unciform, and the astragalus articulates largely with the cuboid, which it does not in the elephants. The presence, however, of five complete and distinct toes to the fore foot, and probably also to the hind foot, is a definite distinguishing character from any known Perissodactyle.

The vertebræ, in their main characters, resemble those of the Proboscideans; though the neck was somewhat longer in proportion. The tail was long and slender.

The head was long and narrow, and in its essential features more resembling that of the rhinoceros than the elephant. It

Fig. 2.



Restored skull of Uintatherium (*Dinoceras*, Marsh).

was elevated behind into a great occipital crest, as in the former, but, unlike that of any other unknown animal, it had developed from its upper surface three pairs of conspicuous laterally diverging protuberances, one pair (the largest) from the parietal region, one on the maxillaries in front of the orbit, and one much smaller than the others, near the fore part of the elongated nasal bones. Whether these were merely covered by bosses of callous skin, as the rounded form and ruggedness of their extremities would seem to indicate, or whether they formed the basis of attachment for horns of still greater extent, either like those of the rhinoceros or the buffalo, must still be a matter of conjecture. Whichever may have been the case, they would have given a very strange aspect to the creature which possessed them, and have been formidable weapons in encounters either with animals of its own kind, or with the carnivorous beasts whose remains have been found associated with it.

The teeth were no less remarkable than the general formation of the skull. The dental formula was $i\ \frac{0}{3}, c\ \frac{1}{1}, p\ \frac{3}{3}, m\ \frac{3}{3} = 34$. The front teeth, or incisors, were, as in modern ruminants, absent above, and in the lower jaw rather small, directed forwards, and forming a continuous series with the still smaller canine. A large, trenchant, enamel-covered tusk, not unlike that of the musk-deer, or Chinese water-deer (*Hydopotes*), descended from each side of the upper jaw, and lay against a singular flattened expansion of the lower border of the ramus of the lower jaw, which has been conjectured to be for the purpose of protecting them from injury, though no such processes are found necessary in the animals above mentioned with similar tusks; and they recall a like conformation of the *Megatherium*, which can have no such function. They must have effectually prevented any stabbing or penetrating action of these weapons. There is some evidence that the tusks were smaller in the females. The molar teeth were six on each side, above and below, placed in continuous series, and separated from the canines by a considerable interval. They were small for the size of the animal, and of simple structure, each having two more or less transverse crests, though those of the upper jaw diverge externally, and meet at the inner border of the tooth in a V-shaped manner.

The brain (as indicated by the size and form of the cerebral cavity, of which a cast has been made and figured by Professor Marsh) was proportionately smaller than in any other known mammal, recent or fossil, and was almost reptilian in its character. (See Pl. CXXXVIII. fig. 3.) It was actually so diminutive (in Marsh's *Dinoceras mirabile*) that the entire brain could apparently have been drawn through the neural canal of all the presacral vertebræ, certainly through the cervicals and lumbars. It was therefore exceedingly unlike that of modern Proboscideans.

These animals, taking the totality of their organisation into consideration, appear to belong to the great Ungulated group, and to hold a position somewhat intermediate to the Perissodactyles and the Proboscidea, though nearer to the former than was at first supposed. This affinity is still further shown by the discovery of other forms, constituting the genera *Bathmodon* and *Metalophodon* of Cope, from an earlier geological horizon, which, with the general structure of the *Uintatheridæ*, retain, in an extremely interesting manner, many primitive characters, common to all early Ungulates, especially the complete number of incisor and premolar teeth. These are forms for fuller information upon which we anxiously wait.*

It should be mentioned that Professor Marsh has made of *Uintatherium* and its immediate allies a peculiar order of mammals, to which he has given the name of *Dinocerata*, while Cope, who formerly included them in the Proboscidea, and placed *Bathmodon* with the Perissodactyla, has now (" Syst. Cat. of Vertebrata of the Eocene of New Mexico," 1875) formed an order called *Amblypoda*, of which the *Dinocerata*, containing the genera *Uintatherium* and *Loxolophodon*, is one sub-order, and the *Pantodonta*, containing *Bathmodon* and *Metalopholodon*, the other. Both, however, admit that they hold a position somewhat intermediate between the modern orders of Proboscidea and Perissodactyle Ungulates, and so stand out, as it were, as broken piers of the bridge by which the gulf which now so completely divides these orders might have been passed over.

The negative evidence (which of course must be received with the greatest caution in palæontology) of the absence of the remains of any of these animals in the Miocene or Pliocene deposits of North America, indicates that the race became extinct, at least in that land, though it possibly may have migrated elsewhere, and perhaps in Asia may have laid the foundation of that family, which first appears in the Old World under the more familiar form of the typical Proboscideans.

While, however, it would be the rashest possible assertion to say that these were derived directly from the Eocene Bathmodons and Uintatheriums, it is not too much to look upon the latter as affording us some indications of the steps by which the process might have taken place, and as such their discovery is one of the most interesting that has been revealed by modern palæontological research.

The history of the North American *Carnivora* may next engage our attention. In the actual condition of affairs, this order is tolerably well represented on that continent. The *Procyonidæ*, or raccoon-like animals, are almost peculiar to it; the bears, and their allies the otters, martens, and skunks are numerous. The dogs also are widely distributed and variously modified. The Felidæ, though tolerably abundant, do not attain the same size and strength as in the Old World, and the *Hyænidæ*, *Protelidæ*, *Cryptoproctidæ*, and the great family of *Viverridæ*, the civets and genettes, are entirely wanting.

As the modern tapirs and peccaries, which pursue their peace-

* It has been shown quite recently that *Bathmodon* is the same as *Coryphodon* (Owen) of the European Eocenes.

ful existence in the deep shades of the tropical American forests, frequently become the victims of the ferocious jaguars and pumas, which prowl in search of prey through the rank vegetation of the river banks, or lie in wait concealed amid the luxuriant foliage of the branches overhead, it is only natural to suppose that the countless herds of tapir and swine-like herbivorous animals which lived in a similar manner amid the ancient Eocene swamps and forests of Wyoming and Colorado, were also destined to furnish subsistence to a tribe of rapacious beasts of form and fashion long passed away. Palæontological research amply shows that this was the case. Side by side with the remains of *Hyrachyus*, *Palæosyops*, and the rest, are found bones and teeth of animals of various size and structure, but of undoubted carnivorous habits. Unfortunately at present most of these are known only by fragments, and not a few of the numerous genera recently described are founded on the evidence of a single tooth!

There are some, however, about which our knowledge has, within the last two or three years, been greatly extended, and which have proved to be of very special interest.

Among these are two genera, called by their describer, Professor Cope, *Synoplotherium* and *Mesonyx*, each represented by a single species, *S. lanius* and *M. obtusidens*; the latter the size of a large wolf, the former somewhat larger, both from the Eocene of Wyoming. These, like so many of the animals of the same period of the world's history, present such a combination of characters, that it is impossible to place them in either of the existing families of the order to which they belong, being in some respects bear-like, in others dog-like, and in some being more generalised than are any existing members of the order. For instance, their claws had not that narrow, compressed, curved, and sharp-pointed form seen more or less in all modern carnivores, and in the highest degree in the most typical or specialised members of the group, the cats; but they were nearly flat, straight, and blunt (from whence it has been conjectured that they were adapted for an aquatic life), and two bones of the carpus, the scaphoid and lunar, which in all existing carnivora (even including the seals) are united to form a single bone, were distinct from each other, as in the majority of mammalia. The lower canine teeth were placed very close to the fore part of the jaw, which appears to Professor Cope "a special modification for peculiar habits, which," he says, "I suspect to have been the devouring of the turtles, which so abounded on the land and in the waters of the same period. The slender symphysis could most readily be introduced into the shell, while the lateral pressure of the upper canines with the lower would be well adapted for breaking the bony covering of those reptiles."

In the character of the molar teeth, of which there were a considerable number resembling one another in form, these animals, and many others less perfectly known, resemble the well-known *Hyænodon* of Europe, a lost type of carnivorous animal first found in the Upper Eocenes of Europe, but abundant also in America at an apparently later age. The members of this group of carnivores are all characterised by long and somewhat slender jaws, containing a series of teeth one behind the other, each being in its form a repetition of the one before it, as in many of the existing predaceous marsupial animals. The greater differentiation of the characters of the teeth and the shortening of the jaws, with corresponding increase of the force with which they can be closed, seen in the highest forms of modern carnivores, is one among many examples of progressive adaptations conducing to more complete efficiency in performing the functions of life. These Eocene carnivores also (according to Cope) show a primitive character in the tibio-astragalar articulation, or "ankle-joint." "The astragalus is flat, and the applied surfaces are nearly a plane, and without the pulley-shaped character seen in existing carnivora, as dogs, cats, and, in a less degree, in the bears and in other mammalia with specialised extremities, as *Perissodactyla*, *Artiodactyla*, &c. The simplicity of structure resembles, on the other hand, that found in the opossum and various *Insectivora*, *Rodentia*, and *Quadrumana*, and in the *Proboscidia*, most of which have the generalized type of feet. The structure indicates that the carnivorous genera named were plantigrade—a conclusion which is in conformity with the belief already expressed, that the mammalia of the Eocene exhibit much less marked ordinal distinction than do those of the Miocene or the recent periods. It is, indeed, questionable whether some of the genera here included in the carnivora are not gigantic *Insectivora*, since the tibio-tarsal articulation in many, the separation of the scaphoid and lunar bones in *Synoplotherium*, the form of the molars, and the absence of incisor teeth in some, are all characteristic of the latter rather than the former order."

The Miocene carnivorous animals found associated with the herbivorous Oreodons of Dakota are more perfectly known, many of them having been well worked out and figured some years ago by Leidy. The most remarkable are several species of *Hyænodon*, a genus already mentioned as found in the Upper Eocenes and Lower Miocenes of France, and also of the south of England; but one of the American species (*H. horridus*, Leidy) is larger than any of its European congeners, its skull (which, as Leidy remarks, is not like that of any existing carnivores, but something intermediate between that of a wolf and an opossum) fully equalling that of the largest individual of the black bear (*Ursus Americanus*); other species were not larger than a fox. These were the last survivors of a group notably different from any now existing.

The remaining American carnivores of the Miocene and more recent ages can be, as far as they are known, referred to one or other of the groups into which the order is now divided. The dog-like forms were abundant throughout the Miocene and Pliocene ages. But in the earliest period more generalised types were met with, assigned to the well-known European genus *Amphicyon*, which differs from the true dogs in the more tuberculated character of its molars, and the presence of the last upper tooth of this class, which is missing in the modern *Canidæ*, and also in the more bear-like structure of its limbs. Various modifications of *Felidæ* were also abundant, the most remarkable in the Miocene period belonging to that group (*Machærodus* or *Depranodon*), with immensely developed sabre-like upper canine teeth, which flourished throughout such an extensive period of time and in so many parts of the world: in the sub-Himalayan region; in Miocene and Pliocene epochs in various parts of Europe, and almost down to recent times in England, as we know by the teeth found in Kent's Hole; in South America, where remains of its largest and most powerful form (*M. neogæus*) have been found in the caves of Brazil and in the alluvial plains of Buenos Ayres; and again in the Miocene of the North American territories. Why this form, so highly specialised for its mode of life, once apparently the dominating type of the whole order throughout the world, should have entirely disappeared, and given way to the more modestly armed modern tigers and leopards, is not very easy to explain.

From the time of the extinction of the sabre-toothed cats in North America, to the present period, other forms more like those now existing continue to prevail, none, however, equalling in size those of the Old World lion or tiger; but of the other families of the carnivora little has hitherto been found. *Ursidæ* and *Mustelidæ*, except in Pleistocene deposits, are very rare; and, what is more remarkable, remains that can with certainty be referred to the *Procyonidæ*, a group whose head-quarters are in America, have not been met with. The families which were previously mentioned as not now existent in that continent are equally unknown in its extinct fauna.

Perhaps the most conspicuous, both on account of their colossal size and their singular conformation and habits, of the animals inhabiting the American continent in the period immediately preceding the one in which we now live, were the great ground sloths, known to us familiarly by the names of *Megatherium*, *Mylodon*, *Megalonyx*, &c. As these animals are peculiarly American, it might have been expected that when the earlier formations of the continent on which they flourished were explored, the remains of similar or at least allied forms would have been brought to light. But hitherto this has not been the case.

Two species of a genus (*Morotherium*, Marsh) allied to *Megalonyx* and *Mylodon*, from Pliocene strata in Central California and Idaho, have been described; but it is a most remarkable fact that not a fragment attributed with certainty to an Edentate animal has been found in any Miocene or Eocene deposit on the North American continent, and therefore (if this negative evidence can be trusted) we shall have to look elsewhere (probably to the Southern American continent) for the region which gave birth to these mighty creatures, and to look upon them as but temporary excursionists into the northern portion of the continent during the Pleistocene epoch.

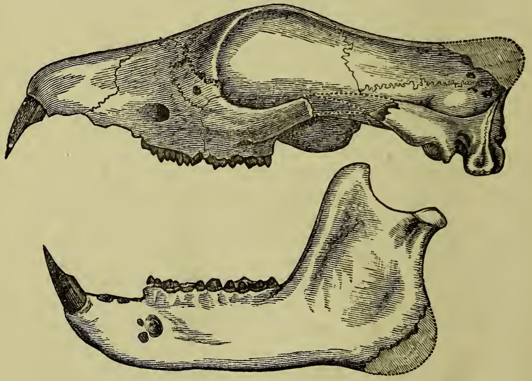
On the other hand, numerous species of the orders *Rodentia*, *Insectivora*, and even *Chiroptera*, and some attributed to the *Marsupialia*, have been found in almost all the hitherto explored fossiliferous deposits down to the Eocenes. Of these time will not suffice to give an account, and this is less important as it is difficult to draw any general conclusions from the fragmentary descriptions of them which we possess at present. I must, however, not omit to call attention to two recently announced discoveries, which, when fully worked out, promise results of considerable interest.

Professor Leidy, in 1868, described a single lower molar tooth from a Tertiary formation, supposed to be Miocene, of Shark River, Monmouth County, New Jersey, apparently of Ungulate affinities, and to which he gave the name of *Anchippodus riparius*. Subsequently a lower jaw of a very anomalous character, from the Bridger Eocene, with large rodent-like perpetually growing incisors, no canines, and bilobed molars, something like those of Palæotherium, was described by the same author under the name of *Trogosus castoridens*; but comparison with the single molar from New Jersey showed so close a resemblance, that the latter name was withdrawn, and both specimens referred to the first described, or *Anchippodus*.

Other similar forms found in a more perfect condition have been described by Professor Marsh, who at a meeting of the Connecticut Academy, Feb. 17, 1875, suggested that as they could be included in no known order of mammals, they should be placed in a new one, for which he proposed the name *Tillodontia*.

"These animals," Professor Marsh observes, "are among the most remarkable yet discovered in American strata, and seem to combine characters of several distinct groups, viz. Carnivores, Ungulates, and Rodents. In *Tillotherium* (Marsh), the type of the order, the skull has the same general form as in the bears; but in its structure resembles that of the Ungulates. The molar teeth are of the Ungulate type, the canines are small, and in each jaw there is a pair of large scalpriform incisors faced with enamel, and growing from persistent pulps, as in the Rodents. The adult dentition is as follows: incisors, $\frac{2}{2}$; canines, $\frac{1}{1}$; premolars, $\frac{3}{2}$; molars, $\frac{3}{3}$. The articulation of the lower jaw with the skull corresponds to that in Ungulates. The posterior nares open behind the last upper molars. The brain was

Fig. 3.



Skull of Anchippodus (*Tillotherium fodiens*, Marsh), $\frac{1}{4}$, from Marsh, "Am. Journ. Sci. and Art," 1876, Plate VIII.

small, and somewhat convoluted. The skeleton most resembles that of carnivores, especially the *Ursidæ*; but the scaphoid and lunar bones are not united, and there is a third trochanter on the fermur. The radius and ulna, and the tibia and fibula, are distinct. The feet are plantigrade, and each had five digits, all terminated with long, compressed, and pointed ungual phalanges, somewhat similar to those of the bears. The other genera of this order are less known, but all apparently had the same general characters. There are two distinct families, *Tillotheridæ* (perhaps identical with *Anchippodentidæ*), in which the large incisors grow from persistent pulps, while the molars have roots; and the *Stylinodontidæ*, in which all the teeth are rootless. Some of the animals of this group were as large as a tapir. With *Hyrax* or the *Toxodontia* they appear to have no near affinities."

The second recently announced discovery to which I alluded is, that a considerable number of fragments of teeth, jaws, and bones from the American Eocenes, the nature of which for some time was an exceedingly difficult problem, really belong to a low form of the great and important order *Primates*, an order embracing the lemurs, various species of monkeys, and culminating in man himself, and which hitherto has not been known with any certainty (except at least by some equally recent discoveries in France) to have existed in the Eocene period. The evidence, however, on which this announcement, made almost simultaneously by Professors Marsh and Cope, rests, is not very fully before the world. Already more than fifteen genera have been named and described, which are assigned to this group, and their characters are said to be those of a low or generalised form of lemur; while some are compared with the true monkeys. Far more rigid comparisons and carefully balanced deductions are required before we can assign their various species to their correct position, and appreciate their bearings upon the generic history of the Primates. In some of the descriptions at present before us lemur and monkey are used as convertible terms, and yet those who have studied these groups most closely are far from being able to pronounce upon the true relationship even of the existing species, and some even doubt whether they ought properly to be associated in the same order. But this is far too large a subject to discuss in all its bearings at the close of a discourse. I can only indicate it as one which may have much light thrown upon it by the researches of American palæontologists.

I can say nothing now of what is being done by the same persons, in the same regions of the world, with regard to other classes of animals than the one I have hitherto been speaking of. But the great and important discoveries of new forms and new links between old forms have not been confined to the mammalia alone. The knowledge of the past history of birds, reptiles, and of fishes, has likewise been greatly enlarged. The very remarkable discovery of Odontornithes, or birds with true teeth and other reptilian characters, has been made. Numbers of new invertebrates, and a whole world of new fossil plants, have been brought to light.

Apart from the special interest of the individual results, some few only of which I have been able to bring under notice on this occasion, the contemplation of what has been done in American palæontology in the last few years teaches us—First, that the living world around us at the present moment bears but an exceedingly small proportion to the whole series of animal and vegetable forms which have existed in past ages. Secondly, that, notwithstanding all that has been said, and most justly said, of the necessary imperfection of the geological record, we may hope that there is still so much preserved that the study of the course of events which have led up to the present condition of life on the globe, may have a great future before it.—*A Lecture delivered before the Royal Institution, March* 10, 1876.

EXPLANATION OF PLATE CXXXVIII.

Fig. 1. Side view of the cranium of Titanotherium (*Brontotherium ingens*, Marsh).

Fig. 2. Upper view of the same, showing the size and form of the brain.

Fig. 3. Upper view of the cranium of Uintatherium (*Dinoceras*, Marsh), showing the size and form of the brain.

Fig. 4. Fore foot of Uintatherium (*Dinoceras*, Marsh).

Fig. 5. Hind foot of Uintatherium (*Dinoceras*, Marsh).

(All $\frac{1}{10}$ the size of nature. From Marsh's figures.)

REVIEWS.

DARWIN'S LATEST BOOK.*

THERE are not a few, even among Mr. Darwin's most enthusiastic admirers, who consider that the present work is the least attractive of the many volumes which the author of the "Origin of Species" has given to the public. And we agree with them to a certain extent, but only so far as the book possesses little to interest a general reader. Those who look on a vast piece of architectural workmanship, if they are of the general crowd, come away with but the vaguest idea of the building they have been looking at. What care they for the tremendous labour and the important skill which must have been displayed in the putting together of the various stones which make up its entirety. So likewise we fancy that of the many—and they are by no means exclusively men of science—who are lavish in their admiration of Mr. Darwin's writings, there are not a few who consider that this work on the domestication of animals and plants, while it may have cost a vast amount of time and labour in its execution, is nevertheless devoid of the interest belonging to other works by the same author. With the naturalist, however, the position is different. He can see at a glance how vast is the amount of evidence which the author has here collected in support of his doctrine of the evolution of races. The whole time of mankind on the earth is but a unit compared with the vastness of the period during which life has been upon the globe. Yet of the period of man alone, how far have we any evidence that is of value? Say about 3,000 years. That is to say, that we have personal observation—as in the records of ancient Egypt—which extends back about 3,000 years, and which shows us that certain animals—the dog, for example—had been bred as he is now. We must admit, then, how extremely difficult is the task of the natural historian who endeavours to find out within this period evidence which will be conclusive as to the tendency of animals and plants to vary, and by means of *natural selection* to have eventually certain species preserved as being the best adapted to the circumstances, while others as certainly "go to the wall" in the struggle for life, and are thus lost to posterity.

Still, small as the time relatively is, it is wonderful how much material Mr. Darwin has collected in these two volumes. Of course, having reviewed this

* "The Variation of Animals and Plants under Domestication." By Charles Darwin, M.A., F.R.S. Second Edition, 2 vols. London: John Murray.

work on its first appearance, we shall not do more than cursorily glance at the more important additions and alterations which the present edition contains; but we may express our wonder at the amount of matter which is collected on the subjects of the horse, ass, pig, cattle, dog, cat, rabbit, pigeons —especially pigeons—fowls, canaries, hive-bees, and silk-worms. And all this, which covers over 300 pages, and which is exclusively relative to animal life, is valuable original material, collected from various observations, beside those of the author, and dealing most minutely with the anatomical differences—in some instances most important—that are displayed by animals that have originally come from the same pair of ancestors. On the subject of plants Mr. Darwin's book is no less copiously instructive. And on such questions as inheritance, selection, variation, and pangenesis, which Mr. Sorby has recently supported,* and on which the author gives much additional argument to what appeared in the former edition of his work, the book is full as full can be. Perhaps one of the most interesting points in the present edition is the reference to Dr. Brown-Séquard's very remarkable results obtained from experiments on rabbits. These show us most conclusively—for the experiments have been tried on many thousands of specimens —that animals born of parents that have been rendered epileptic by section of the sciatic nerve, are themselves distinctly epileptic. And not only this; but that changes in the shape of the ear, ophthalmia, absence of toes, &c., occur in the descendants of animals in which these conditions have been the result of operation. Assuredly in such a fact as the absence of toes in the descendants of animals whose toes had been destroyed, we have—if the evidence is sufficiently powerful—an important argument in support of pangenesis.

We could quote more largely from the author's interesting researches, but we have done enough to show the great importance of his labours, and to prove the interesting character of the volumes under notice. In one or two instances in which Mr. Darwin has had to refer to papers in this journal, it is to be regretted that he omitted the word "Popular" from the title, as there may be some confusion as to the source referred to. It is to be observed, also, that the volumes are not so large in shape as the former ones; this has been effected by no change in the type, but by cutting down the pages a little. The result has been to make the work far more convenient for reading.

POPULAR CHEMISTRY. †

THERE is a considerable difficulty in reviewing such a book as that before us, from the circumstance that it is intended to play a double part. That is to say, that one is disposed to be neither severe nor unduly favourable in his notice of the work. But if one would honestly and bluntly express

* In his address to the Royal Microscopical Society, published in the "Monthly Microscopical Journal," March 1876.

† "A Class-book of Chemistry on the Basis of the New System." By Edward L. Youmans, M.D. London: Henry S. King & Co. 1876.

his opinion, it must be at once said that, somehow or other, the result of reading it is unsatisfactory. If truth must be told, the volume is one which neither possesses the information that is required by the earnest student, nor places the facts of the science in so popular a position that the unlearned in chemistry may read it with profit. We might take several examples of this tendency of the book, but one or two will suffice. We turned in the first instance to the chapter on Polarisation, as in that we were sure to find the author's popular mode of teaching put to a severe test. But what did we find? Why, that he has just given the usual explanation, and has not attempted anything in the way of genuine popularisation. We confess that if we were unacquainted with physics, and turned to Dr. Youmans' work for an explanation of this complex phenomenon, we should be no wiser after reading his account than we were before. But we will take another example in order to show our readers that we are entirely unprejudiced in the matter, and that Dr. Youmans is absolutely incapable of really popularising a difficult subject. The quotation we will make refers to the theory of *Atomicity* and *Quantivalence*; and Dr. Youmans' remarks on the subject of "variable combining capacity" are as follows:—"The general theory of chemistry now adopted is the outgrowth of preceding theories, and embodies the truths they have severally attained. But it adds an important principle which throws further light upon chemical operations, and serves to organise into a better system the later facts and ideas of the science. The notion of equality between combining elements and of equivalence among their atoms has long been fundamental in chemistry. When the substitution theory arose," &c., &c.

We think we have given enough to create dismay in the minds of all lovers of genuine popular science. However, there is something to be said in the author's favour. He has popularised many important points with considerable success, and he has supplied diagrams and drawings very freely, while withal he has furnished a series of questions at the termination of the book which will, we doubt not, be found extremely useful by the young student.

ANIMAL MORPHOLOGY. *

THE Professor of Zoology in the University of Dublin has long been known as a comparative anatomist of no mean ability, more especially in regard to those classes of the animal kingdom which come beneath the division Vertebrata. But we have not heard of him as one who was specially devoted to the various Invertebrate groups of animals. Yet it is especially on the classes that are included in the Protozoa, Cœlenterata, Mollusca, Annulosa, &c., that we find him writing in the present volume. We note, moreover, that he is modest enough in his preface, for he assumes that his book will form, as it were, a sort of introduction to the works of Rolleston, Huxley, and Flower. However, we fancy that most students will find that

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* "An Introduction to Animal Morphology and Systematic Zoology." By A. Macalister, M.B., Professor of Comparative Anatomy in the University of Dublin. Part I. Invertebrata. London: Longmans. 1876.

the present work will prove for all purposes of examination sufficiently modern and adequately advanced. At least we think so ourselves. The author commences by a series of remarks on protoplasm, general morphology, histology—of the muscular, nervous, and connective tissues—tectology, reproduction and distribution which is given both as to space and time. Of these several chapters we have not much to say. They appear generally good, but the remarks are excessively brief, and in one or two cases are by no means what we should have anticipated. This may be said especially of the author's observations on the subject of muscular tissue. We observe, too, with some pain, that the author does not seem to have accurately understood Mr. Darwin's remarks on the subject of natural selection. At least it seems so to us. For example, Dr. Macalister says: "But while explaining the method, natural selection does not explain the cause. In artificial selection the cause is the presidence and direction of human intellect. In natural selection there is a necessity for predicating the existence of a presidence similar in kind, but grander in degree, as the changes effected by it are greater than those that artificial selection can accomplish." We certainly fail to see the reason alleged, and we should much like to hear the author's opinion more fully on this point.

In regard to those portions of the work which deal with the purely zoological parts of the subject we have only praise to offer. We think the author has endeavoured to state points that will be useful to the reader, to the exclusion of new matter which refers to but single points, and which has not yet been clearly established. We note, too, that in almost every instance where a new fact is stated, the author's name which is most directly associated with it is given in parentheses.

It may be stated, as points objectionable in the work, firstly, that the author is accustomed to giving a great series of new words which *he* thinks—but we do not—serviceable from being an abbreviated mode of obtaining information; secondly, that the work is lamentably badly illustrated, the total number of woodcuts being about forty in number.

COLLECTING NATURAL HISTORY OBJECTS.*

A VERY valuable series of papers, which originally appeared in "Science Gossip," is here reprinted with their authors' names attached, and the whole has been issued under the editorship of Mr. J. E. Taylor, who contributes the first chapter on the subject of Geological Specimens. We may say at the outset that the work is eminently a practical one, and we doubt not will have a very large sale among all dilettanti naturalists. It is generally clear and to the point, while wherever illustration is required it is given. It is remarkable, too, that in a book which is written by a great number of naturalists, the general plan has been followed by all; and this

* "Notes on Collecting and Preserving Natural History Objects." Edited by J. E. Taylor, Ph.D., F.L.S., F.G.S. London: Hardwicke & Bogue, 192, Piccadilly. 1876.

is a circumstance for which we must thank the editor in no small degree. Besides the Geological chapter to which we referred above, and which is written in an admirably popular and withal accurate style, the following sections make up the volume:—"Bones," by E. Elwin; "Birds'-eggs," by T. Southwell, F.L.S.; "Butterflies and Moths," by Dr. Knaggs; "Beetles," by E. C. Rye, F.L.S.; "Hymenoptera," by J. Bridgman; "Land and Freshwater Shells," by Ralph Tate, F.G.S.; "Flowering Plants and Ferns," two chapters, by J. Britten, F.L.S.; "Grasses," by Professor Buckman, F.G.S.; "Mosses," by Dr. Braithwaite, F.L.S.; "Fungi," by Worthington Smith, F.L.S.; "Lichens," by the Rev. James Crombie, F.L.S.; and "Sea-weeds," by W. H. Grattan. Anyone who is at all familiar with natural history-workers will see at a glance that in most instances—as, for example, in those of "Fungi," "Lichens," "Mosses," &c.—the names of the authors selected are those of the very highest living authorities in their several departments. In each chapter we find ample instructions as to the implements to be employed, and the mode of using them in the capture of the several forms. But we are also informed as to the time and place in which the particular "quarry" is to be best taken; and, further, there is ample information supplied as to the best and most charitable mode of killing live animals, and as to the manner in which they shall be mounted after death. And now—having said so much in praise of this little book—must come a word or two in disparagement. It is to the technicalities which the authors employ that we object. Of course in some instances it is impossible to substitute common names, but in many cases the substitution is perfectly practicable and ought to have been adopted, and we trust will be followed in the next edition. The only chapter with which we can find no manner of fault in this respect is that which opens the work, and is by the editor. But certain others—we shall not name them—are as full of technicalities as it is well possible to be. Still the work is the only one of the kind, and it is *tout ensemble* an excellent one.

FERMENTATION.*

THIS book will, we doubt not, prove of interest to chemists; but we fear that the practical element is wanting which would have recommended it to a large number of readers who are engaged in our large breweries. Indeed, we think that the author has been forced to spin out his matter, so to speak, in order to make a book upon the subject. We note, too, that he is clearly ignorant of the lives of the lower groups of fungi, and we think that for this he is to blame; for assuredly, even though he may have been ignorant of the development of common *Mucor* at the period when he began his work, the study of the subject in any treatise on fungology need not have taken up very much of his time. It is also to be observed that his figures are insufficient, and the microscopic observations are taken altogether

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* "On Fermentation." By P. Schützenberger, Director at the Chemical Laboratory of the Sorbonne. London: Henry S. King & Co. 1876.

second-hand from other artists. But when we have said this we have expressed ourselves to the fullest extent that adverse criticism can urge us. There is a good deal that is most interesting in the book; and although, of course, it does not clearly define many of the points, it is needless to observe that this results from the fact that few of the great questions—those which have formed the discussions that took place some years ago between Baron Liebig and M. Pasteur—are even at the present moment in anything like a settled condition. M. Schützenberger, who is evidently of German origin, though attached to the laboratories of the Sorbonne, begins of course with the history of the subject, and treats us to several pages on this matter. And here we might observe that dealing with questions like the present one is utterly impossible in anything like the space at the writer's command. So this chapter might well have been omitted. The following are the headings of the several subsequent sections:—Alcoholic Fermentation; Alcoholic Ferments; Composition of Ferments; Function of Yeast; Action of various Agents on Alcoholic Fermentation; Can nothing but Alcoholic Yeast excite Alcoholic Fermentation? Mannitic Fermentation of Sugar; Lactic Fermentation; Ammoniacal Fermentation: Butyric Fermentation and Putrefaction; Fermentation by Oxidation; Application of the Researches of M. Pasteur; Albumenoid Substances; Origin of Ferments; Proteids; Soluble Ferments; and last, but not least, On the Origin of Ferments. There is nothing new in this book to those who have regularly read the "Comptes rendus" for the past twelve years. But for English readers alone there will be found an immense mine of valuable information, with reference more especially to the ideas of M. Pasteur. It is singular, however, that much as is the knowledge that has been obtained on the subject of fermentation generally, so little should have been done in the investigation of those diseases which are known to have their origin in the presence of certain fermenting materials within the body of the animal affected. On this point the author says—referring to a page which he has been quoting from a former work on M. Davaine's inquiries—that "during the ten years that have elapsed since this was written, skilful experimentalists, guided by the same ideas, among whom I may mention Pasteur himself [researches during the cholera epidemic], have studied this subject with great care; and yet I must admit that there has been no result from these inquiries; the question of the etiology of infectious diseases has made no important advance; the observation made by M. Davaine remains without any additional support." Those who remember the nature of Davaine's inquiries are aware that they were made upon animals infected with malignant boils.

On the subject of the French work that has been done on the question of fermentation, the reader will find ample details given, while the style is generally a most pleasant one; and though the book is deficient in its practical details, nevertheless it is an important, and therefore a welcome, addition to our literature.

AMERICAN SURVEYS.*

THE report of Captain Jones contains an account of a reconnaissance made in 1873, in North-Western Wyoming, with a view of finding a good and shorter route from the South, by the Wind River Valley and Upper Yellowstone to Montana; and from which it appears a district of considerable economical importance would be opened out, both as regards the agriculture and the mineral resources of the districts, especially of Montana, which is considered to be one of the most productive mining regions of the West. It is accompanied by a general map and forty-nine trail-maps, showing the physical features of the country passed over, of which an account is given in the text. The volume also includes reports on the Botany, Entomology, and Geölogy of the district explored. The latter, by Professor Comstock, gives a general review of the various formations observed, and the results of the dynamical forces which have operated, as well as an elaborate and interesting account of the Hot Springs and Geysers which form so remarkable a feature in the Yellowstone district. These phenomena of post-volcanic activity must have been very extensive and of long duration, for there is evidence of the existence of large tracts of ancient hot-spring deposits in many places where there are no signs of present or very recent activity, showing the amount of vigour that has been displayed at a remote period, though geologically recent, and the persistency of the heat to the present epoch. The first number of the Bulletin is mainly occupied with a series of interesting accounts of the ancient remains, ruins, and works of art found during the progress of Geological Survey of the Territories, which are fully illustrated by maps, plans, and numerous plates of the different points explored. The bead ornaments used by the prehistoric people of Utah and Arizona for personal decoration appear to have been of four kinds—shells, chiefly the genus Oliva; earthenware beads, turquoise very rare, and pendants made usually of stone or silicified wood, but occasionally of pieces of pottery. Some such ornaments as these are still worn among the Mojaves, Moquis, Pueblos, and Zuñians of Arizona and New Mexico. The second part is entirely ornithological, and contains studies of the American Falconidæ and the Ornithology of Gaudeloupe Island, by R. Ridgway.

A volume of 500 pages† records the progress of the operations of the geological survey in the more rugged mountain portions of Colorado during the season of 1874, and shows the continued activity of Dr. Hayden, the members of his staff, and the other collaborators who have contributed to its pages. Dr. Hayden gives a résumé of the geology of the eastern base of

* "Report upon the Reconnaissance of North-Western Wyoming, including Yellowstone National Park." By Captain W. A. Jones. With Appendix. Washington, 1875. "Bulletin of the United States Geological and Geographical Survey of the Territories." Vol. ii. parts 1 and 2. Washington, 1876.

† "The United States Geological and Geographical Survey of the Territories, being the Report of Progress for the year 1876." By Dr. F. V. Hayden. Washington. 1876.

the Colorado range, and of the Elk Mountains, as well as an account of the characters, extent, and probable age of the lignitic group, and also an interesting chapter on glacial lakes and lake-basins, in which is shown the effects of glacial action and other denuding agencies of erosion in giving form to the rocky ranges of the West, where in some cases more than 10,000 feet of sedimentary strata have been swept away at a comparatively modern period. Professor Lesquereux contributes an important paper on the cretaceous and lignitic floras of North America, with descriptions of some new species. There are also reports on the geology of the Elk range, Middle Division, San Juan district, and Nebraska, by Messrs. Holmes, Peale, Endlich, and Aughey, besides reports on zoology, archæology, and topography. These reports are accompanied by nearly ninety maps, plans, sections, and plates, illustrative of the various subjects treated of in this volume.

GEOLOGICAL MANUALS.*

THE issue of a second edition of the useful "Guide to the Geology of London and the Neighbourhood," by Mr. Whitaker, in so short a period after the publication of the first, proves that it has been very acceptable to a large number of persons who are interested in and anxious to acquire a general knowledge of the character and arrangement of the various formations which occur in and near the metropolitan area. This new edition contains references to the memoirs of geological survey, where details of the sections noted may be found.

The second edition of the "Rudiments of Geology," by Mr. Sharp, has been thoroughly revised, and several important additions have been made throughout. The subject-matter is generally clearly and concisely treated, and so arranged, together with some tables and woodcuts, as to render it a more useful hand-book for the student of the science.

The little work by Mr. Mello is intended to give a general sketch of the geology of Derbyshire. With the exception of some mammalian gravels and bone-caves of Pleistocene age, the geology of the county is comparatively simple, consisting of the Trias, Permian, and Carboniferous formations; the latter occupy about two-thirds of the whole area. With the exception of a small inlying portion of the Leicestershire coal-field in the extreme south of the county, the coal measures of Derbyshire, with an area of about 230 square miles, are a continuation of the great Yorkshire coal-field. It is partly to the underlying millstone grit and mountain limestone, aided by denudation, that Derbyshire owes the wild and picturesque features of its dales and the long stretches of moorland, while the wooded and more cultivated southern portion occurs upon the softer Triassic strata.

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* "Guide to the Geology of London and the Neighbourhood." Second edition. By W. Whitaker, B.A., F.G.S. London: E. Stanford, 1875. "Rudiments of Geology." By S. Sharp, F.S.A., F.G.S. London: E. Stanford, 1876. "Hand-Book to the Geology of Derbyshire." By the Rev. J. M. Mello, M.A., F.G.S. London: Bemrose & Sons.

The work by Professor Green * must be classed among the advanced manuals for the teaching of geological science, and is not only intended to serve those students who desire to know as much of the science as any man of culture may be reasonably expected to possess, but also to form a text-book for the school and lecture-room, the author feeling that any work on natural science which is intended for educational use, and to assist the mind to reflect and reason, must not only state clearly the results arrived at, but must also put forth the methods by which they have been obtained. This idea seems generally to pervade the work. In this volume the facts bearing on the fundamental groundwork of the science are only attempted, and include the principal points of lithological and dynamical geology. A short chapter on the aim and scope of geology, with a sketch of its rise and progress, is followed by a description of the chief rock-forming minerals and a classification of rocks according to their two chief modes of origin, and their petrological characters; the volcanic, metamorphic, and granitic rocks being separately described. Denudation and its results, both as forming new deposits, and its effects in the shaping of the surface, are fully treated, as also are the various changes in position which rocks have undergone since they were originally deposited. The last two chapters are devoted to the original fluidity and present condition of the interior of the earth, with some remarks on speculative geology, and how the various changes of climate inferred by the geologist have been brought about. The clear and philosophic manner in which the various subjects are generally treated, and the numerous original and well-selected illustrations, as also the many notes and references to other works where more detailed information can be found, will render this book a valuable addition to the other treatises on the science.

BOTANICAL NAMES.†

FOR the general reader this book will not have special interest, but for the botanist it possesses many charms. It is an account of how botany first came to be studied, even before the Christian era, and during the last eighteen centuries; and added to this is a short description, in alphabetical order, of the origin of the several terms that have formed generic titles. The first part of the book is the most interesting, as it is also the best done. The second part is the more imperfect, but its importance cannot be overrated. We have seen nothing in the shape of a popular account of the meaning of botanical expressions but that of Mrs. Lankester, who, it will be remembered, was the joint editor of the last edition of Sowerby's botany. The account she has given, too, has to do with the significance rather of the popular names than the more specially scientific ones. Still it was unquestionably a very excellent addition to Dr. Syme's labours. Mr. Alcock has endeavoured to give the exact meaning which

* "Geology." By Professor A. H. Green, M.A., F.G.S. London: 1876.

† "Botanical Names for English Readers." By R. H. Alcock. London: L. Reeves & Co. 1876.

each scientific name had at the time when it was first applied; and as it is his first attempt, of course every allowance must be made. At the same time, we think that many of his explanations are very far-fetched. Let us take, for example, *Bartsia odontites*; in this case he says that Pliny asserts that a portion of the stem was used for curing tooth-ache, hence the specific title. Again, *Borago* he attributes to the words *Cor* and *ago*: also *Camelina*, which he derives from *chamai*, the ground, and *linon* flax, which we think few who know the plant will agree to. Finally, the word *Euphorbia*, the derivation of which is attributed to Euphorbus, a physician to the King of Mauritania; and why, save for the resemblance of the names, remains a perfect mystery.

We have chosen these few examples, but we might have added very largely to the list had we chosen to do so. It seems to us that the author has not been very learned on the subject, and that he has overlooked many valuable sources of information in the botanical writings of the past couple of centuries. Still we must own that he has done a good work which we wish every success to, and we trust that when it comes to a new edition the author will enlarge and otherwise improve it. The history of botany, which precedes the dictionary, is most excellently given.

ANTI-DARWINISM*.

WE are somewhat amused, but not at all surprised, at a lawyer's coming forward in the ranks of Mr. Darwin's assailants. And, what we might have very naturally anticipated, viz. that the effort would be of a kind to resemble the mode of argument that is termed "special pleading," is clearly the sort of thing which we have to deal with. The arguments in the book before us are those which we have often heard urged against Mr. Darwin, which are almost universally of one class. We use the word "almost" because we wish to exclude one work, which is certainly a clever one, but which is opposed to the Darwinian theory. It might be fairly asked, how could a gentleman whose legal practice admits of his writing a book on the "History of the Currency" have had the opportunity of fairly considering the arguments in favour of Mr. Darwin's doctrines? He might, to be sure, have taken some half-dozen works, and skimmed them through in his chambers. But what a preparation would this be for the consideration of the grandest problem that has ever presented itself to the mind of man! Mr. James Maclaren presumes, we suppose, that Mr. Mivart has not made the most formidable onslaught on the Darwinian theory, and he therefore merely uses a few of his arguments. But indeed it is not worth while treating at length of this book, which, however fairly it may pretend to have been written, is the weakest and most one-sided argument in existence. Let us take one example of the author's powerful mode of argument as to the ape-origin of man :—

* "A Critical Examination of some of the principal Arguments for and against Darwinism." By James Maclaren, M.A., Barrister-at-law. London: E. Bumpus. 1876.

"There is nothing, however, in this view of the progress of man which shows that he has descended from an ape-like ancestor; in fact, the general diffusion of man over the earth (and many evolutionists suppose that this diffusion took place before man had acquired language) is quite inconsistent with a development from an ape-like creature, as all existing man-like apes are, as we have seen, extremely local, capable of existing in a few spots only."

The other arguments are like the above, and may be left to the reader's judgment for their annihilation. But we would ask the author one question on the subject he has written on. Suppose a man, with no particular religious belief, who admits the possibility of there being a Creator, were to ask, "How has man come on the globe? There is much evidence of his kinship with the lower animals; whence, then, comes he?" What would be his reply? We suppose we shall have to wait his next volume for an answer.

OUR FIVE SENSES.*

WE think that Herr Julius Bernstein is to be congratulated upon the style of this book. We are much pleased with the manner in which he introduces us to the various subjects he has undertaken to describe, and indeed altogether his language is clear and to the point. He has taken the five senses, *i.e.* sight, hearing, touch, taste, and smell, to describe, and with each of them he has dealt fairly—more especially with the senses of sight and hearing. Of course it must not be imagined that he has told us anything new; that would be a great mistake. On the contrary, there are many points unnoticed in this book which Dr. Tyndall has expatiated at length upon in his various writings. Still he has stated the main points in an admirably lucid style. He gives more than 100 pages to the sense of sight alone, and he supplies numerous illustrations—some of them too familiar, perhaps—of the different experiments that are to be made on the subject of vision. In one of these, however, we note that a series of figures which are intended to look of different sizes though actually of the same size, are slightly misplaced, so that people view them at considerable disadvantage. There is absolutely nothing new in the entire volume, but still it will prove, we do not doubt, a very useful addition to our popular natural philosophy libraries.

SHORT NOTICES.

Bulletin of the United States National Museum. Washington: 1875.—In this number Professor E. Cope has published a check-list of North American Batrachia and Reptilia, with a systematic list of the higher groups, and an essay on their geographical distribution.

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* "The Five Senses of Man." By Julius Bernstein, Professor of Physiology at the University of Halle. London: Henry S. King & Co. 1876.

Mr. Brough Smyth, Secretary for Mines for Victoria, has prepared a detailed descriptive catalogue of rocks, minerals, and fossils illustrative of the geology, mineralogy, and mining resources of the colony, exhibited on behalf of the Government, at the Philadelphia International Exhibition, 1876.

Telegraphy. By W. H. Preece, C.E., and J. Sivewright, M.A. London: Longmans, 1876.—This is one of Messrs. Longmans' excellent manuals. It is essentially a practical work, intended as an introduction to Culley's famous treatise, which was noticed in these pages a few years ago. The authors explain that the work is intended alone for those who have been engaged in practical telegraphy. To such we recommend the volume. Its cuts are excellent.

Exercises in Electrical and Magnetic Measurement, with Answers. By R.E. Day, M.A. London: Longmans, 1876.—These are carefully prepared, and they will be found useful by all who are engaged in the practical study of electrical and magnetic apparatus.

Evolution of the Human Race from Apes &c., &c., unsanctioned by Science. By T. W. Jones, F.R.S., F.R.C.S., Professor of Ophthalmic Medicine and Surgery in University College, London. London: Smith & Elder, 1876.—Here is an admirably written and spirited essay against Darwinism. We recommend it to our readers' consideration because we are sure that they will peruse it with pleasure, and will put it down more convinced of the truth of Darwinism than they were before.

The Moon and the Earth. By T. M. Reade, C.E., F.G.S., President of the Liverpool Geological Society. London: Hardwicke & Bogue, 1876.—Those of our readers who have looked at the admirable series of representations in Messrs. Nasmyth & Carpenter's fine treatise on the Moon will not agree with Mr. Reade's conclusions. Still there is a good deal to be said for his ideas as to the comparison between the earth's and moon's appearances. It certainly does appear as if there were no disturbing influence in the case of the moon as there is in the earth's atmosphere. At the same time one is bound to ask how so much volcanic agency as once existed at the moon's surface could have operated without the presence of oxygen? In any case Mr. Reade's little pamphlet is a most interesting one.

We have received:—"Gumpel's Patent Rudder" (London: J. Pettitt, 1876); "Vis Inertia at the Post Office" (fourth edition, Longmans, 1876); "Why do we Breathe?" by P. Black, M.D.; "Annual Report of the Marine-Hospital Service of the United States, 1873;" "Annual Report of the Supervising Surgeon of the Marine-Hospital Service of the United States, for 1874;" "Science made Easy;" a series of Familiar Lectures," &c., by T. Twining. Parts I., II., III., and IV. (London: Chapman & Hall.)

THE LOAN COLLECTION OF SCIENTIFIC APPARATUS AT SOUTH KENSINGTON.

THOSE who have not yet visited the collection of apparatus at South Kensington can have little or no idea how vast and important it is. Those who have, however, will readily pardon us for not doing more for our readers than giving a general sketch of the exhibition, which is on the whole the finest that has ever been made, or in all probability ever will be made within our ken. At the same time we think that those who have superintended the work of exhibiting the various apparatus have been to blame in two distinct ways: 1st, in exhibiting many instruments that have not the slightest claim to novelty, but are merely exhibited because the particular makers had influence over those who had to do with the matter. 2nd, for the abominable manner in which some of the apparatus is exhibited, being in fact of no use whatever to students who desire its close examination. There is also a deficiency of labels explanatory of the objects. It is all very well to urge upon the spectator that he can readily obtain a catalogue, though unfortunately such cannot be had; but an exhibition which is got up by Government should not have been wanting—as this one is sadly—in explanatory labels. Those who had the management in their hands should have taken a lesson from the highly satisfactory arrangement that is invariably pursued in the South Kensington collection. The following notes are taken from a series of valuable articles which appeared in the "Academy" (May 20, and subsequent numbers):—Following the order of arrangement in the building, the first Section is that under which "Educational Appliances" are classed; and here the most striking feature is the collection forwarded from Russia by the Committee of the Pedagogical Museum; the models by Strembitsky, which belong to this series, are extremely good and very instructive. Germany has contributed largely to this section, and several private firms have sent well-filled cases of apparatus for teaching physical science. England is fairly represented, but from France there is nothing, and from Austria, Italy, Holland, and Belgium a few objects only.

In the next section, "Applied Mechanics," there is much to interest every one: the original models of steam-engines and other machines of Watt; the original models of Stirling's air-engine, and Trevithick's locomotive engine atented in 1802; the "Rocket" and "Puffing Billy," brought out from their retreat in the Patent Office Museum; Brahmah's first hydraulic press; the steam-engine used on Dalswinton Lake in 1788; and the original engine of the steamboat *Comet*. The Royal School of Mines and the Council of

King's College exhibited good collections of models illustrative of the principles of mechanics, and some equally good models come from Germany. Among the specimens of naval architecture are models of the *Faraday*, the *Staunch* gunboat, and the *Castalia*; and beyond these, in the passage leading to the western courts, will be found an interesting collection, exhibited by the Trinity House, under the sub-head "Lighthouses and Fog-signals." In Section 9, "Magnetism," are some of the apparatus used by Faraday, and the loadstone from which he first obtained the induction spark; apparatus used by De la Rive; the greatest natural magnet known; self-registering instruments from Kew, and instruments exhibited by the Admiralty, among which are patterns of those issued to the Arctic Expedition. In Section 10, "Electricity," the special collection sent by the Postmaster-General to illustrate the history of electric telegraphy will attract attention, and so will the original apparatus used by Faraday and by De la Rive. Other objects of interest are Nairne's early electrical machine; Armstrong's hydro-electric machine; Gramme's magno-electric machine; Cooke and Wheatstone's first working telegraph; the instruments used in the Atlantic Cable Expeditions of 1853 and 1866; the original Wheatstone Bridge; copies of the first German telegraphic apparatus constructed in 1809, of the first needle telegraph, and of the electro-magnetic telegraph apparatus of Gauss and Weber of Göttingen, made and used from 1833 to 1838; and a polar-light apparatus by Professor Lemströn.

Under Section 1, "Arithmetic," will be found an old calculating-machine invented by S. Morland, and made in 1664; two machines designed by Lord Mahon, and made in 1775–77; the portion of Babbage's calculating machine put together in 1833; the "Napier Bones," made about 1700, and used by the originator of logarithms for performing division and multiplication; Sir William Thomson's tide-calculating machine; and several calculating-circles from Germany. In Section 2, "Geometry," there are many good drawing-instruments and models; and in Section 3, "Measurement," an interesting collection of standard measuring-apparatus, contributed by the Standards Department of the Board of Trade. In the latter section are also Whitworth's delicate measuring-instruments, by which differences of one ten-thousandth and even of one-millionth of an inch can be appreciated, and his new hexagonal surface plates; here, too, are Joule's apparatus; contributions from the Geneva Association for Constructing Scientific Instruments; Boulengé's electric chronograph; Bashforth's clock-chronograph; and, among the instruments for measuring time, a watch which was twice carried out by Captain Cook, and again by Captain Bligh in the *Bounty*, on which occasion he accompanied the mutineers to Pitcairn's Island, and finally, after many vicissitudes, returned to England in 1843. In Section 4, "Kinematics, Statics, and Dynamics," is a collection of Gravesande's apparatus, and a series of Kinematic models exhibited by the K. Gewerbe-Akademie, Berlin. In the northern portion of the west gallery the classification of the catalogue has hardly been followed, and the objects exhibited are grouped more with reference to the purpose for which they are employed: thus astronomical instruments, meteorological instruments, land survey instruments, mining survey instruments, naval survey instruments, and apparatus used in deep-sea exploration, each form separate groups. This section of the collection possesses so many objects of interest that it is hardly possible to do justice to it in a short notice, and we can only enumerate some of the most important, which will be found in the catalogue under Sections 11, 14, 15, 16. These are: an astrolabe of Sir Francis Drake; a quadrant of Tycho Brahe; Newton's telescope; a quadrant of Napier; a transit instrument made by Lingke, of Freiberg; a telescope by Huyghens, and eye-pieces ground and polished by him; Sir W. Herschel's 7-foot telescope, and his 10-foot Newtonian reflecting telescope; the Galileo and Torricelli relics from Florence, including two telescopes made by the former; Baily's apparatus; Gauss' pendulum for demonstrating the rotation of the earth; Gravesande's heliostat; a complete Transit of Venus equipment; Colby's compensation-bars used in the measurement of the basis in the north of Ireland in 1827 and on Salisbury Plain in 1849; Ramsden's 36-inch theodolite and his smaller 18-inch one, which was set up over the cross at St. Paul's Cathedral; the surveying instruments in use on the Prussian survey; instruments and apparatus used by H.M.'s ships in deep-sea exploration; mining instruments; and a fine collection of meteorological instruments. In Section 15, "Geography," may be noticed some of Livingstone's instruments; MS. plans of Livingstone, Burton, Speke, Grant, and Stanley; MS. journals of Cook, Franklin, and Parry; the log of the *Bounty*; the Ordnance Survey collection of maps; specimens of the survey of Palestine; and some good models of ground. In Section 16, "Geology," will be found the apparatus employed in Sir James Hall's celebrated experiments; specimens of the work of the Geographical Survey; illustrations of the Sub-Wealden boring; original sketches by Dr. Buckland; Davy's safety-lamp; and the latest improvements in goniometers. In Section 18, "Biology," are Van Leuwenhoek's microscope; Van Musschenbroek's microscope; the instruments used by Hooker, Dawson, Turner, and Brown, and the instruments recently described by the Rev. Mr. Dallinger, in the *Monthly Microscopical Journal*; and apparatus used in the several branches of physiological research. In Section 13, "Chemistry," are the apparatus employed by John Dalton in his classical researches; balances used by Sir Humphry Davy and Dr. Joseph Black; Faraday's apparatus for the condensation and liquefaction of gases; Dr. Andrew's apparatus for proving that ozone is a condensed form of oxygen; and a collection from the Master of the Mint, illustrating the processes of gold and silver assaying, including an old cupellation furnace supposed to have been used by Sir Isaac Newton. The collection of physical apparatus is of very great interest; under the head of "Molecular Physics," Section 5, are a small model of Colladon's new air and gas compressors used for the St. Gothard Tunnel; Von Guericke's air-pump and the two celebrated Magdeburg hemispheres exhibited at Ratisbon in 1654; the first air-pump with two barrels; Thilorier's apparatus for liquefying carbonic acid; the apparatus employed by Dr. Andrews in his researches on the continuity of the gaseous and liquid states of matter; and a series of diagrams illustrating the improvements made in the air-pump. Under "Sound," Section 6, will be found the apparatus employed by Colladon in 1826 for ascertaining the velocity of the transmission of sound through water; the double-siren used by Helmholtz in his researches on sound; Le

Roux' apparatus; the first obliquely-strung upright piano patented by Wornum in 1811; models of ancient Egyptian pipes; Tyndall's apparatus; and a stand of apparatus illustrating the progress of Æolian principles. In Section 7, "Light," are: Sir David Brewster's early stereoscope; a camera obscura of Sir Joshua Reynolds; the original form of Brewster's kaleidoscope made in 1815; spectroscopic apparatus used by Sir John Herschel; a fine collection of spectroscopic instruments by Browning and others; Wheatstone's polar clock; Crookes' radiometers; the first photograph taken on glass by Sir John Herschel; the second daguerreotype, a view of the Palais d'Orsay, taken by M. Daguerre in 1830; Herschel's experiments on the action of light on different kinds of salts; and the results of Dr. Forel's experiments in the Lake of Geneva on the penetration of the sun's rays in the waters of the lake. In Section 8, "Heat," may be noticed the original Lavoisier calorimeter, the apparatus used by Professor Tyndall in his researches on the absorption of radiant heat by gases and vapours; Wedgwood's pyrometer; a Musschenbroek's pyrometer; Siemens' pyrometer; apparatus used by Tyndall, De la Rive, and others; and a very fine collection of thermometric instruments.

SCIENTIFIC SUMMARY.

ASTRONOMY.

THE SUN.—Secchi has published a report on solar phenomena during the second half of the year 1875. He finds a *minimum* of activity, the culminating epoch of which would be in March 1876. The number of protuberances has been very varying, from 2 or 3 one day to 10 or 12 the next. The jets of hydrogen were usually straight, even if attaining 2′ or occasionally 3′ in height; an indication of great tranquillity. The chromosphere was low at the equator, but often very elevated (24″ to 30″) at the poles, from the displacement of *maxima* in that direction.—We are sorry to find that the Solar Observatory at Bothkamp has ceased to exist, the Director, Dr. Vogel, having accepted a post at the new observatory of Berlin. Dr. Lohse, whose work has been published together with Vogel's, thinks there is evidence of a subordinate period of 50 days in the eruptive action of the sun. From the drawings of the Spectroscopic Society of Italy he has made out a curve determined by the times of observation combined with the area of the protuberances, and found that, besides *maxima* and *minima* corresponding with those of the spots, it showed a well-marked period of 50 days during 1871, 1872, and the beginning of 1873; but subsequently the whole solar activity became so small, from the 11-years' period, that these secondary *maxima* became undistinguishable. From spectroscopic observations Lohse is led to infer that the electro-negative elements which are not traceable in the sun may exist in the outer layers of the corona. He finds many of the as yet unknown dark lines in the more refrangible end of the solar spectrum, in that of *a Herculis*, and also, though feebler, in that of *Betelgeuse*; but they are not perceptible in *Arcturus*.—Lord Lindsay has presented to the Royal Astronomical Society four folio and ten 4to. MS. volumes, containing the very valuable series of sun-spot observations by the late Mr. Carrington, between 1853 and 1871, which was used for determining the present received values of the position of the solar axis and the drifts in the photosphere. They were recently bid for at an auction by the Society, but purchased by a bookseller, from whom Lord Lindsay subsequently obtained them.

Venus.—It is to be hoped that advantage has been taken of the favourable situation of this lovely planet, to investigate the markings of her surface, and the irregularities of her terminator. These were once the subject of considerable discussion between Schröter and Sir W. Herschel, and, on the part of the latter, of some little asperity; after which the enquiry seemed to languish till it was taken up by De Vico, but still left in a position hardly corresponding with our advance in the knowledge of most of the other planetary surfaces. There is no doubt considerable difficulty in the investigation; and failures, even with large and powerful instruments, have been numerous. No question, however, exists as to the occasional visibility of brighter specks, and still more frequently of dusky shadings, which stand on multiplied testimony; and there is no adequate reason for not adopting the numerous representations given by De Vico and his associates at Rome, from their observations between 1839 and 1841, with a $6\frac{1}{4}$-in. object-glass by Cauchoix, and in a sky of whose pellucid clearness we can form very little idea in the perturbed and agitated condition of the English atmosphere. It seems strange, however, that these results should never have been verified at the same observatory with the very superior $9\frac{1}{3}$-in. Merz achromatic in the hands of so eminent an astronomer as Secchi; nor have they, so far as we are aware, ever been continuously investigated elsewhere. It is very true that the enlarged apertures of the present time, however keen in definition, are baffled to a great extent by the intense glare of this vividly reflective globe; but it is no less true that this disadvantage might be obviated by the use of a pale screen-glass, or by employing the hours of daylight, which indeed De Vico found alone suitable for his purpose. And where there are so many instances of the occasional detection of these spots, even with very small apertures, there is much encouragement for the perseverance of amateurs.

The Recent Transit.—The reduction of the English observations is proceeding vigorously under the superintendence of Captain Tupman. The amount of work involved has been marvellous. About 5,000 transits of stars were taken for the correction of clock and instrumental errors. The longitudes of the stations at Mauritius and Rodriguez were measured from Suez by Lord Lindsay with fifty chronometers; and Mr. Burton has made more than 6,000 microscopic measures to determine the optical distortion of the photoheliographs. It is self-evident that a considerable time must elapse before the final result, even of the British observations, can be made known; and it is not as yet decided whether a separate value shall be deduced from these, or whether they are to be combined with the results of all other nations.

The Moon.—The investigation of the surface of our satellite, if we may judge by the number of published observations, makes no very rapid advances, and seems to be little attended to at the principal European observatories. Several amateurs, however, are doing good service in working out details. Many interesting points remain to be discussed as to the probable mode of formation of the lunar surface; in fact, very little has hitherto been done in what may be termed selenology. The materials are as yet not very abundant, but sufficient perhaps for a rough draught, which would be tested and corrected by future observation. There can be little reason for doubting that the key to the lunar configurations lies in the analogy of the eruptive processes of the earth; but important modifications would necessarily be introduced by the dissimilar conditions of the two globes—by the great difference in the force of gravity, in atmospheric pressure, possibly in internal temperature, and even in the cohesion and fusibility of materials; for though there is certainly every probability in favour of the assumption that the earth and her companion are identical as to composition, it cannot be considered as absolutely demonstrated, or perhaps demonstrable. The supposition that the same elementary forms prevail throughout the solar system has been rendered indefensible by spectroscopic enquiry. Not to insist on minor planetary differences, which as yet may be uncertain in their indication, unsolved mysteries yet hang around the central region of light and fire, and the two outermost planets; of which, prior to the introduction of spectrum-analysis, the telescope could give no kind of information. However, in dealing with selenological enquiries, we can make no other assumption but that of identity with terrestrial materials; and we should only be justified in abandoning it by difficulties on that hypothesis insuperable.

Whatever may be the theories of modern geologists, or whatever changes may yet await some of their conclusions, one thing seems evident, that the eruptive force which has moulded the surface of the moon into its present strange configuration has been decaying down to either comparative or absolute extinction. It is certainly not very material whether our generation may be contemporary with its expiring efforts, or with a subsequent state of quiescence; but it is a question not without much interest; and few observers would not hail with pleasure an opportunity of witnessing the activity of a lunar volcano. However, it is still *sub judice* whether anything of the kind has occurred since the invention of the telescope; and there is more difficulty than might be supposed in forming a reliable opinion, partly from the inaccuracies and mistakes of the earlier observers, partly from the deficiencies of existing maps, and partly from the backwardness to supply those deficiencies at the hands of the possessors of the powerful instruments of the day. Close investigation and careful drawing is required, and that under several angles of illumination; and though photography may render most important service, as that of an eye which never omits anything, yet the circumstances would be very exceptional which would give to its renderings the keenness and certainty of ocular inspection. Each mode may help the other. It is of course among the minutest craters, and, according to that great authority Schmidt, among the fissures or cracks, that we must seek for the evidence of remaining chemical life. But change of perhaps a less intelligible nature may be detected among the multitude of light-streaks and brilliant patches which variegate the fully-enlightened moon with such perplexing intricacy. There is strong evidence of altered brightness in some places, and it is much to be wished that some careful, patient observer would undertake the task of giving us a portion at least of a map of the full moon.

Mars.—The next opposition of this planet in 1877 will, it is believed, give a very reliable value of the solar parallax; and the Astronomer Royal has prepared a chart of stars suitable for comparative observation.

The Minor Planets.—In the "Berliner Astron. Jahrbuch" for 1878 Professor Tietjen has published the approximate places of 144 of the 160 small planets for the present year, and accurate opposition ephemerides of 71.—*Flora.* From corresponding observations of this body in 1873, 41 in the N. and 40 in the S. hemisphere, Galle has deduced a value for the solar parallax of 8″·873.

Jupiter.—Flammarion has made an extensive series of observations on the satellites. He thinks that his results show a difference in their intrinsic nature. IV is less luminous, though larger, than I and II, and very variable, from 6 to 10 mag., probably not from permanent spots, but atmospheric changes. III seems invariable, at 5·9 mag. I and II are about 6·8 and 7 mags., both slightly variable and very white. I can be seen by day, when IV is invisible. Daylight alters the relative lustre, the size of the larger discs gaining by night. I has sometimes appeared smaller than II. Reduced to equal surfaces, the light would stand I, II, III, IV; sometimes II brighter than I. Order of variability, IV, I, II, III. There is some evidence that the changes in IV may arise in part from its always turning the same face to the primary; but its brightness appears complicated with atmospheric disturbance.

Saturn.—A series of micrometrical measures of the satellites has been made with the Greenwich 12¾-in. equatorial.

Uranus.—Mr. Isaac W. Ward, of Belfast, appears to have repeatedly succeeded in glimpsing by averted vision the two brighter satellites with an object-glass of only 4·28 inches by Wray, the estimated positions being subsequently found very fairly accordant with those in Marth's tables. Webb also believes that he has caught sight of *Titania* with a very fine 9·38 silvered glass "With" speculum.

Stars and Nebulæ.—Baron Dembowski has been continuing the measures of double stars, which have for many years given him a high pre-eminence among observers. The following periods of revolution which he has adopted for comparison may be found of use:—ζ *Herculis* (Dunér), 34·22 years. η *Coronæ* (Wijkander), 41·58 y. ξ *Scorpii* (Thiele), 49·05 y. ξ *Ursæ Maj.* (Hind), 60·68 y. ζ *Cancri* (O. Struve), 62·4 y. 70 *Ophiuchi* (Schur), 94·37 y. Σ 3062 (Schur), (112·64 y. ξ *Bootis* (Hind), 168·9 y. γ *Virginis* (Thiele), 185·01 y. δ *Cygni* (Behrmann), 415·1 y. *Castor* (Thiele), 996·85 y. Σ 634 = 19 *Camelopard.* Hevel = P IV 269. This pair is called by Smyth an elegant object, 5½ and 9 mags. light yellow, pale blue. The components are certainly in motion, having been, according to Σ, nearly 37″ apart in 1827, but now only about 20″. O. Struve considers it undecided whether this approach may be due to opposite proper motions or orbital connection: he thinks they may be at a comparatively small distance from us, and may show a sensible parallax, for which the pair is very favourably situated. *Sirius.*—Mr. Erck has observed a considerable diminution in the angle of position of the *comes.* Auwers has computed a period of 49·399 years, but the agreement is latterly less satisfactory. 303 double stars have been measured with the 8¼-in. Alvan Clark refractor at Rugby. *Algol.*—Schönfeld has combined 55 observations of *minima* of his own with 183 by Schmidt, and 50 by Argelander, all between 1846 and 1875. The resulting period is 2·867288 days, probable error ± 7m. The most probable duration of the variation, from 2·2 to 3·7 mag., is 9¼h., diminishing most rapidly 1h. 26m. before *min.*, and increasing most perceptibly at an equal brightness 1h. 47m. after *min.* Dr. Schjellerup has published a translation of the Arabic "Description of the Fixed Stars," by Abd-alrahman al-Sûfi, in the tenth century, who evidently observed their magnitudes with minute exactness: so that his labours are unrivalled even to the epoch of Argelander's "Uranometria Nova." Dr. Schjellerup has given a very interesting comparative table of the magnitudes of the principal stars, from Ptolemy, Sûfi, and Argelander, which shows that on the whole they are as accordant as could be expected.—Observations of the nebulæ, and measurements of the positions and distances of neighbouring nebulæ and stars, have been continued at Parsonstown.—The original telescopes of Galileo have been sent over from the Florentine Museum to our Scientific Loan Exhibition.—Colonel Cooper's Observatory at Markree, containing the great 13·3-in. Cauchoix achromatic, which has been long suffering greatly from neglect and decay, is now being restored as far as possible under the care of Dr. Doberck, and good work may be expected from it.

BOTANY AND VEGETABLE PHYSIOLOGY.

The Growth of the Male and Female Flowers of Valisneria Spiralis.—In reference to a note on this subject in our number for April, Mr. W. Morrison, the Honorary Secretary of the Dundee Naturalists' Society, sends us the results of some experiments that were conducted by Mr. J. Hood, one of the Vice-Presidents of that Society. He says: "The plants have been in cultivation since 1865, and during that time there have been female flowers four seasons and male flowers on two seasons; the latter in the years 1869 and 1875. The plants continued in flower from June to about the middle of August. The male flowers, including peduncle, never exceeded $1\frac{1}{2}$ inches in length, and the length of the portions above the soil ranged from $\frac{5}{8}$ to 1 inch. From their shortness they may sometimes escape observation. The results of the observations on the rapidity of growth in female flowers were:—

1875					
June 30	8 P.M.	measured plant, and found it to be 2 inches in length			
July 1	9·30 A.M.	increase	$11\frac{7}{8}$	inches in	$13\frac{1}{2}$ hours
" "	8 P.M.	"	$6\frac{5}{8}$	" "	$10\frac{1}{2}$ "
" 2	9·30 A.M.	"	11	" "	$13\frac{1}{2}$ "
			$31\frac{1}{2}$		$37\frac{1}{2}$

The observations were not continued further. It will be seen that the flower increased in length $29\frac{1}{2}$ inches in $37\frac{1}{2}$ hours, and that the increase was much more rapid by night than during the day. Seedling plants are growing in the aquarium from last year's seed."

Experiments on the Digestive Power of Plants.—It is stated by a contemporary that Dr. M. T. Masters has been experimenting on the functions of the nectaries formed by the small cup-shaped petals of *Helleborus*, and finds that they absorb or digest nitrogenous substances, repeating in all respects the phenomena of the leaves of *Drosera* and *Dionæa.*

The Zanzibar Copal and the Copal-tree.—A very interesting paper was that recently read before the Linnean Society (April 20) on this subject. It was a letter from Dr. Kirk, of Zanzibar, which was read by Dr. Hooker. It referred to the identification of the modern copal-tree, *Trachylobium Hornemannianum,* with that which yielded the Zanzibar Copal or Gum Animi, now found in the earth on the East Coast of Africa, and often where no copal yielding tree now exists. Little doubt now rests as to the identity of the semi-fossil with the living tree, inasmuch as bijugate leaf, flower-bud, flower, ovary and stamens, characteristic of the latter, have been discovered in the so-called Animi. Dr. Kirk is inclined to account for their difference in quality by a molecular or chemical change in the buried material; improving it thereby, and as a consequence increasing its market value.

Floral Æstivations.—At the meeting of the Linnean Society (June 1, 1876), the Rev. G. Henslow read a paper on "Floral Æstivations," in which he explained the origin of eight kinds, more particularly referring to the new term "half-imbricate." This latter he applied to a large number of cases ranging from perfectly regular to extremely irregular and zygomorphic flowers of the pea and snapdragon. The author added a note on a new theory of the cruciferous flower, based on a quinary type. He also disputed the tenability of Chorisis in the pairs of long stamens, regarding their occasional union as indicative of evolutionary advance and not retrogression; as cohesion is a subsequent stage to freedom, except in the rare cases of atavism indicated by solution and dialysis. The justness of Pfeffer's view of the corolla of *Primula* being an outgrowth of the andrœcium he calls in question, giving several reasons in support of this adverse opinion.

African Coffee-plants.—A paper was read before the Linnean Society (April 20), on the African species of the genus *Coffea,* Linn., by Mr. W. P. Hiern. As now restricted this genus belongs to the Old World, attributed American species being referred to other genera. Out of seven Indian species one formerly was cultivated, but has been superseded by African plants. The author distinguishes thirteen species as indigenous to the African continent, and two to Mauritius and Bourbon. Of the former, two kinds are found in East and Central Africa, the remainder ranging along the West Coast. The ordinary commercial coffee, he shows, grows wild in Abyssinia and other parts of Africa; and as to the celebrated Mocha coffee, he regards it as but a doubtful variety of the ordinary sort. A technical description is for the first time given of Liberian coffee, *C. liberica.* This only recently has acquired importance, having been introduced into England in 1874 by Mr. W. Bull, the horticulturist. Already, however, its fame is spreading far and wide among coffee-planters, especially those of Ceylon. Its introduction there has been regarded as a great boon, and justly so; its qualities far surpassing any kind hitherto known. This undoubted distinct species of coffee is robust, hardy, and very productive. It is large-leaved and big-berried, and the latter in flavour and aroma are very superior to the common *C. arabica.* As it thrives at lower altitudes and in districts inimical to the latter, its commercial importance hereafter is likely to be very great. Other useful qualities attributed to it time and experience may test.

Peculiar Food of Ants.—Mr. Francis Darwin read a paper "On the Glandular Bodies on *Acacia sphærocephala* and *Cecropia peltata,* serving as

Food for Ants." The structures in question were discovered by Mr. Belt (Nicaragua), and subsequently further observations made by Fritz Müller (Brazil), while Mr. Darwin has more particularly entered into their minute composition. In *Acacia* they are of two kinds, small, somewhat flattened, pear-shaped bodies, which tip six or seven of the lowermost leaflets of the bipinnate leaves. In *Cecropia* cylindrical bodies are developed in flat cushions at the base of the leaf stalk. Mr. Darwin shows the microscopic structure in all of these to be homologous in kind, cellular, protoplasmic, and containing oil-globules. He infers, moreover, they bear a relation to the serration-glands of Reinke, in certain cases afterwards being converted into stores of nutriment, which undoubtedly the ants live on, and in their turn protect the trees from the ravages of the leaf-cutting ants.—Linnean Society, June 1.

Pythium Equiseti.—Great interest is just now attached to this curious parasite, and hitherto it has not been recorded as British. Dr. Sadebeck, of Berlin, described the plant last year as a new species of Pythium, parasitic upon *Equisetum arvense.* Mr. W. Smith says ("Gardener's Chronicle") it bears a considerable resemblance to the bodies discovered last year, and referred by me to the secondary condition of the potato fungus. It ultimately appeared that Dr. Sadebeck also last year found a similar parasite infesting and destroying living potato plants near Coblenz, and at the time he referred the Equisetum and potato parasites to the same fungus, and on seeing my micro-photographs he doubtfully threw out the suggestion that all three fungi might possible prove to be the same with each other. On these insufficient grounds a report was spread in this country that the organisms described by me were the same with Dr. Sadebeck's Pythium Equiseti, and the "Journal of Botany" for March last stated, in reference to Pythium Equiseti, that it had "lately been attempted to connect this fungus with the oospores of *Peronospora infestans.*" Dr. Sadebeck can hardly be said to have made such an attempt, for in a very kind letter that he wrote me on March 23 last he said the presumed identity was a mere "supposition," thrown out in a preliminary paper, that he was without experiments from which to form a definite conclusion, and that he had not been able to infect the potato plant artificially with the Pythium. Dr. Sadebeck's excellent paper, and the evident strong external resemblance of his newly-discovered plant to mine, made me extremely desirous of seeing the Berlin plant, but on writing to Dr. Sadebeck to this effect he replied that he had no specimens. It therefore only remained to look out for the parasite here, and I was fortunate enough to enlist the good services of Mr. B. D. Jackson, F.L.S., who sent me some capital specimens of *Equisetum arvense* from Snodland, Kent, on April 25. The first piece of Equisetum I examined under the microscope displayed the presence of fungus spawn ramifying amongst the tissues ; so, from experience gained of the habits of some of the lower fungi, I half submerged the specimens of Equisetum and kept them covered up in a dark place. In ten days the Equisetum plants were dotted inside and out with gelatinous patches, and every patch was a mass of Pythium Equisetum. Though bearing a strong resemblance to the early condition of the bodies found by me in the Chiswick potatos, yet Pythium Equiseti is clearly not the same. Mr. Berkeley, who has seen both plants, writes me that he considers them "decidedly different." I have been unable to infect the potato plant with the Pythium or the Equisetum with my potato oospores. My experiments, therefore, agree with the results obtained by Dr. Sadebeck, and the two parasites may be considered different.

The Black Knot of Plum and Cherry-trees is the subject of an important memoir, illustrated by plates, in the "Bulletin of the Harvard University." The paper is illustrated by three beautiful plates, showing this disease in various stages, and the whole structure, development, and fructification of *Sphoria mortosa* of Schucinitz, the fungus which produces this black knot, which so deforms and injures plum and cherry-trees throughout the Northern States and Canada. The remedy is the knife or the axe. For prevention Dr. Farlow recommends the extirpation of choke cherry-trees, upon which the pest largely breeds in the vicinity of Boston. Farther west it would all the more be necessary to destroy all the wild plum-trees (*Prunus Americana*), which are fearfully infested.

Mr. Meehan's Explanation of his Attack on Mr. Darwin.—At a recent meeting of the Academy of Philadelphia Mr. Thomas Meehan remarked that the American correspondent of "Nature" had characterised some recent remarks of his on fertilization by insect agency as an attack on Mr. Darwin. He thought the members of the Academy would bear him out in the statement that the facts and observations he had from time to time offered were submitted in no spirit of antagonism to Mr. Darwin, but often favoured as much as they opposed views held by that distinguished gentleman. Even those who were avowed partisans of Mr. Darwin felt it necessary to strengthen their position by searching for new facts. Surely the mere student, who was willing to wait till the evidence was all in, might offer the facts as he found them, without being liable to the charge of direct antagonism.

CHEMISTRY.

Detection of Chicory in Coffee.—Professor Wittstein, who has a long paper on the adulteration of coffee in the "Chemical News" (May 12), states that Mr. J. Horsley, some time ago, proposed the following process for the detection of chicory in coffee:—If to a much diluted decoction of chicory a solution of bichromate of potash be added, no sensible reaction will take place. If, however, we subject to this same reagent a decoction of pure coffee, its colour will immediately darken, and become brown similar to porter. This is, therefore, an easy method of distinguishing between the two, provided they are separate. In mixtures the determination of the impurity becomes much more difficult. In this case a dilute decoction is made of a weighed quantity of the suspected mixture. It is then to be heated to boiling and treated with the solution of bichromate of potash. A few decigrammes of copper sulphate are next added, and the solution is again to be boiled, whereupon a dark brown flocculent precipitate will be formed. The depth of its colour depends on the quantity of coffee in the mixture; and we have thus, by comparing this precipitate with a similar one of the same quantity of pure coffee, an approximate method of examining our mixture quantitatively.

The Reaction of Biliverdin.—This is a subject to which Dr. Thudicum has recently been devoting his attention. He read a paper upon it at the meeting of the Chemical Society on May 4. After stating that the cause of the yellow colour of the skin of persons suffering from "yellow jaunddice" was bilirubin, whilst the dark colour of the so-called "black jaundice" was due to the presence of biliverdin, he proceeded to describe some derivatives of the latter substance. *Monobrominated biliverdin*, $C_8H_8BrNO_2$, was prepared by passing bromine vapour mixed with dry air over finely powdered biliverdin until it ceased to be absorbed, and the product was then heated to 100°C. in a current of dry air. It is a black powder, insoluble in ether, and very little soluble in alcohol. It is soluble in sulphuric acid, but is precipitated on dilution with water. It is soluble in caustic soda, being precipitated again in brown flocks by acetic acid. *Hydro-biliverdin* is formed on treating a solution of biliverdin in dilute caustic soda with sodium amalgam. It dilutes alcoholic solution, gives a spectrum showing an absorption-band overlying the line F equilaterally, and totally different from the broad band between E and F shown by solutions of hydro-bilirubin. We are not quite prepared to accept Dr. Thudicum's views as to the pathology of the disease.

The Manufacture of Sulphuric Anhydride.—A paper on this important practical subject was read by Dr. W. Squire before the Chemical Society on April 20. Dr. Squire, after giving a sketch of the history of the manufacture of sulphuric acid, described the process for preparing the anhydride. The vapour of ordinary sulphuric acid is passed through a white-hot platinum tube, whereby it is almost completely decomposed into water, oxygen, and sulphurous anhydride: the mixed gases, after passing through a leaden worm to condense the greater portion of the water, are completely dehydrated in a leaden tower filled with coke, over which a stream of concentrated sulphuric acid is allowed to trickle. The dry mixture of oxygen and sulphurous anhydride is now passed through platinum tubes heated to low redness, and containing fragments of platinised pumice, when the gases recombine to form sulphuric anhydride, which is condensed in a series of Woulffe's bottles.

Experiments on the Sugar Beet have been lately carried out by MM. Fremy and Dehérain, which show (Comptes rendus, April 24) that saline solutions identical in composition act very differently upon beets accordingly as the roots plunge into the solutions themselves, or as the latter merely occupy the pores of the soil. On planting beets of different origin in identical conditions as to soil, manure, and watering, roots are obtained differing in their yield of sugar. An excess of nitrogenous manure lowers the percentage of sugar in all beets, but those of a superior strain preserve still such a quantity of sugar that they may be advantageously treated. To produce from a given surface the maximum of sugar under conditions advantageous alike for grower and manufacturer, we must depend above all on a judicious selection of the seed.

Calcareous Alabaster from Mexico.—M. A. Damour says, in a paper read before the French Academy, on May 8, and quoted by the "Chemical

News," May 19, that this material is known in commerce as the onyx of Tecali. It varies in colour from milk-white, yellowish white to pale green, certain samples displaying brown veins shading into red. It takes a fine polish. Its specific gravity is 2·77. It is readily and entirely soluble in nitric acid. Its composition is—

Carbonic acid	43·52
Lime	50·10
Magnesia	1·40
Ferrous oxide	4·10
Manganous oxide	0·22
Water	0·60
Silica	traces
	99·94

Sulphur in Coal Gas.—M. A. Virigo states that ("Chemical News," May 12) he found, in portions of 100 cubic feet of gas made at Odessa, respectively 2, 1·81, 1·9, 2·01, and 2·2 grms. of sulphur. He readily detected the presence of sulphurous acid in the air of rooms lighted with this gas, and demonstrates its ready conversion into sulphuric acid in contact with moist organic matter, such as cotton yarn.

Experiments as to Insecticides on the Phylloxera.—M. Dumas' book on this subject has been published, and has been reviewed in the "Chemical News" of May 2, from which we take the following remarks:—"As regards the phylloxera of the roots it is found that the sulpho-carbonate of potassium, of which more than 20,000 kilos. have been already used, is a rapid insecticide, the only one which certainly destroys the phylloxeras fixed upon the roots, and which affords at the same time an efficient nourishment to the vine. The sulpho-carbonate of sodium offers similar advantages as an insecticide only. The sulpho-carbonate of barium being an anhydrous salt, and sparingly soluble, is recommended by its resistance to the action of oxygen and to that of carbonic acid, which renders it a poison less rapid, but of an effect more durable. As to the winter-eggs the heavy oil of gas-tar, and especially the so-called oil of anthracen, seems to be the most suitable agent for anointing the branches and for destroying the winter-eggs. The application of gas-tar to the branches and of sulpho-carbonates to the roots is best performed in the months of February and March.

Death of Dr. Letheby.—The "Analyst" says: "We have to announce, with deep regret—a regret which will be shared by our readers—the death, somewhat suddenly, of Dr. Letheby. He had been unwell for some weeks, his complaint being, we believe, inflammation of the lungs. Dr. Letheby was too well known in the chemical world to require any lengthened obituary notice at our hands. We may, however, mention that he was an early member of the Chemical Society; that he took his M.B. degree in 1843, became Ph.D. and M.A. in 1858; that amongst the numerous appointments which he had held were those of Medical Officer of Health and Public Analyst for the City of London; and that he was the author of numerous scientific and hygienic works. He died in his sixtieth year."

The Production of Bromine in America.—The "American Journal of

Pharmacy" states that bromine was produced in America as early as 1846, for photograph purposes. With the decline of the daguerreotype process the manufacture of bromine also ceased. In 1866 the employment of bromides in medicine renewed this branch of industry, the element being obtained from the mother liquors of the salt-works at Tarentum and Natrona in Alleghany. In 1868 an increasing demand led to its production in Pennsylvania, Ohio, and Western Virginia. Between the years 1867 and 1873 the amount produced rose from 5,000 to 88,000 kilogrammes. Up to the year 1870 the yield merely sufficed to supply the demands of the United States, and during that year bromine was first exported. Since that date the amount produced has steadily increased, and has so largely exceeded the demand that no new factories are now erected.

GEOGRAPHY.

Return of Lieutenant Cameron.—At the meeting of the Royal Geographical Society, which took place on April 11, Lieutenant Cameron was gallantly received. The meeting was held in St. James's Hall, which was crowded by a large and fashionable audience. The chair was taken by H.R.H. the Duke of Edinburgh, who in the course of his opening remarks congratulated the navy on the fact that a member of it should have accomplished so great a feat as Cameron's journey from sea to sea was. Lieutenant Cameron then gave a brief *résumé* of his journey from the East Coast to Ujiji, and thence across Lake Tanganyika to the West Coast. The main features of this narration have already been made known to the public through the pages of the "Geographical Magazine" and "Proceedings" of the Royal Geographical Society. Regarding the interesting question of the outlet to Tanganyika, he stated that there was no place to which the Lukuga could flow except into the Lurwa, and that native information corroborated this view. Apart from the great difference between the volume of water of the Lualaba at Nyangwe and that of the Nile at Gondokoro, the levels proved conclusively that the two rivers could have no connection. He had seen a good deal of the slave-trade, and observes that the Portuguese are the principal agents in the trade, the Arabs as a rule buying only enough slaves to act as porters and servants. The only effectual way of putting an end to slavery was to open up Africa to legitimate commerce by utilising the magnificent water-systems of the interior. Sir Henry Rawlinson expressed, on behalf of the council of the Society, a high opinion of Cameron's services, which, besides their geographical importance, were equally interesting to the politician, the merchant, and the philanthropist. He had been almost continuously on the tramp for two years and eight months, during which he had been exposed to every kind of hardship, and had travelled over 3,000 miles. His observations, which numbered over 5,000, were copious, elaborate, and accurate. Among the most noticeable of the results of his expedition were his exploration of the southern half of Lake Tanganyika and his discovery of the outlet, and his demonstration of the probable identity of the Congo and Lualaba. Another important result was the discovery of a new political power, that of Kasongo, apparently the greatest chief in equatorial Africa. Any measures for suppressing the slave-trade would probably be carried on through his agency. Lieutenant Cameron had tracked the atrocious traffic in slaves to its fountain-head, and had thus rendered a great service to civilisation as well as to geography. In conclusion, Sir Henry announced that the council of the Society had awarded the principal gold medal of the year to Lieutenant Cameron. After a few remarks from Dr. Badger and Sir Alexander Milne, the latter of whom pronounced Lieutenant Cameron to be a credit to his profession, H.R.H. the Duke of Edinburgh moved a vote of thanks for the paper, and the meeting broke up.

GEOLOGY AND PALÆONTOLOGY.

Relations between Reptiles and Mammals.—Professor Owen has lately described a carnivorous reptile, named by him *Cynodracon major*, which has the compressed sabre-shaped canines of the lion of the genus *Machærodus*, and resembles carnivores both in the canines and incisors. In the lower jaw the bases of eight incisors and of two canines (very inferior in size to the canines of the upper jaw) are visible, and the canines are separated by a gap from the incisors. In this character, as in the number of incisors, the fossil resembles a *Didelphys*. The left humerus is 10½ inches long, but is abraded at both extremities; it presents characters—in the ridges for muscular attachment, in the provision for the rotation of the forearm, and in the presence of a strong bony bridge for the protection of the main artery and nerve of the forearm—which resemble those occurring in carnivorous mammals, and especially in the Felidæ, although these peculiarities are associated with others having no mammalian resemblances. Professor Owen discusses these characters in detail, and indicates that there is, in the probably Triassic lacustrine deposits of South Africa, a whole group of genera, many represented by more than one species, and all carnivorous, which have more or less decided mammalian analogies; and to them he gives the general name of *Theriodonts.*

The Petrified Forest of California.—At a recent meeting of the Troy Scientific Association, Dr. Ward delivered an address on the Petrified Forest of California. He considered the peculiar fracture of the fallen petrified trunks their most suggestive and important peculiarity, since they are broken up somewhat symmetrically in a manner that might happen to wood rendered brittle by charring or perhaps by partial petrifaction, but could hardly be conceived as occurring to ordinary wood or stone.

The Brain of Dinoceras seems to have been remarkably small. The *Dinoceras,* which has been recently discovered by Professor Marsh in the Eocene beds of Wyoming, nearly equalled the elephant in size, but the limbs were shorter. The head could reach the ground, and there is no evidence that it carried a proboscis. Professor Marsh figures the skull in his second memoir, entitled "Principal Characters of the Dinocerata." "The brain-cavity in *Dinoceras* is perhaps the most remarkable feature in this remarkable genus.

It proves conclusively that the brain was proportionately smaller than in any other known mammal, recent or fossil, and even less than in some reptiles. It was, in fact, the most reptilian brain in any known mammal. In *D. mirabile* the entire brain was actually so diminutive that it could apparently have been drawn through the neural canal of all the presacral vertebræ, certainly through the cervicals and lumbars."

Remains of Coryphodon and Dinocerata.—The examination of a series of Mammalian remains, obtained from the Eocene deposits of Wyoming, Utah, and New Mexico, has led Professor Marsh to infer that the genus Bathmodon described by Professor Cope clearly belongs to the genus Coryphodon of Owen. This is especially important and interesting, as the geological horizon of the remains is essentially the same in both countries, and the American specimens promise to clear up many doubtful points in regard to the animals themselves. The characters shown in the skull and limbs of Coryphodon indicate that this genus was essentially Perissodactyle, and represents a distinct family which may be called Coryphodontidæ. Professor Marsh has described the principal characters of a well-marked group of gigantic mammals, which are abundant in the lower Miocene deposits on the eastern slope of the Rocky Mountains. These animals, the Brontotheridæ, of which four genera are known, equalled in size the gigantic Eocene Dinocerata, and resembled them in some important features, but differed from them in having but a single pair of horn cores and no crest around the vertex; the structure and number of the teeth were also quite different, and do not belong to the same order, but constitute a distinct family of Perissodactyles.

Birds with Teeth.—The same author has also given an account of a remarkable group of birds with teeth, obtained from the cretaceous beds of Kansas, where the associated vertebrate fossils are mainly Mososauroid reptiles and Pterodactyls. They constitute a sub-class, Odontornithes, comprising two orders—the *Ichthyornithes*, having the teeth in sockets, biconcave vertebræ, a keeled sternum, and wings well developed, represented by Ichthyornis and probably Apatornis, and the *Odontolcæ*, with the teeth in grooves, the vertebræ as in recent birds, a sternum without keel and rudimentary wings, represented by Hespeornis. The occurrence of toothed birds in England has been described by Professor Owen from the London clay of Sheppy.

American Fossil Fishes.—Professor J. S. Newberry has further described the structure and relations of Dinichthys and other fossil fishes from the Devonian and Carboniferous strata of Ohio. The most striking feature of *Dinichthys*, apart from its great size, is the dentition, which is massive and peculiar, and offers some remarkable and suggestive points of resemblance with Coccosteus among fossil, and Lepidosiren among living fishes, besides which there is a singular correspondence between the ventral shields of Dinichthys and Coccosteus. From the examination of a large amount of new material, Professor Newberry remarks that the discovery of Dinichthys is a matter of interest, not simply because it adds another and the most gigantic to a strange extinct group of fishes, but also because it serves as a connecting link between several genera of Devonian Placoderms, of which the affinities have been somewhat obscure, viz. *Coccosteus* and

Pterichthys with *Asterolepis* and *Heterostius*; and more especially because it shows a relationship to exist between these peculiar fishes and the anomalous living *Lepidosiren*, and thus showing that probably a parallel line ran upward from the Devonian Placodenus to the other living branch of the Dipnoan family, now represented by Lepidosiren and Protopterus.

A new Trilobite.—Dr. S. T. Barrett has described, in the "American Journal of Science" (March 1876), a new and interesting form of Trilobite, *Dalmanites dentata*, so named from the peculiar dentate margin of the cephalic shield. Each thoracic segment terminates with a slender terete spine, curved outward and backward. This species is found in the Lower Helderberg formation, near Port Jervis, Orange County. Some of the layers of the rock are mainly made of its remains.

On a Gigantic Bird from the Eocene of New Mexico.—Professor Cope exhibited recently to the Philadelphia Academy of Science a tarsometatarsus of a bird, discovered by himself during the explorations in New Mexico, conducted by Lieutenant G. M. Wheeler, U. S. A. The characters of its proximal extremity resemble in many points those of the order *Cursores* (represented by the *Struthionidæ* and *Dinornis*), while those of the distal end are, in the middle and inner trochleæ, like those of the *Gastornis* of the Paris Basin. Its size indicates a species with feet twice the bulk of those of the ostrich. The discovery introduces this group of birds to the known faunæ of North America, recent and extinct, and demonstrates that this continent has not been destitute of the gigantic forms of birds, heretofore chiefly found in the Southern Hemisphere faunæ.

MEDICAL SCIENCE.

The Physiology of Hearing.—The "Berlin Journal of Chemistry" is responsible for the following facts, which it gathers from a medical journal. It states that Herr Urbantschitch calls attention to the fact that if a watch be held at a little distance from the ear, the ticking is not heard uniformly, but there is a swelling and diminishing of the sound. If held at such a distance as to be scarcely audible, the ticking will come and go, being at times perceived distinctly, but at times becoming wholly inaudible, as if the watch were being moved to and from the ear. This variation in perception is not always gradual; it is sometimes sudden. The same holds good for other weak sounds, as that of a weak water-jet, or a tuning-fork. Since breathing and pulsation have not the least influence on the phenomenon, the interruptions of the sensation must be attributed to the organ of hearing itself; our ear is unable to feel weak acoustic stimuli uniformly, but has varying times of fatigue. To decide finally where the peculiarity lay, M. Urbantschitch made both ear-passages air-tight and applied a tuning-fork and a watch to the head. The sounds seemed not continuous, but intermittent. The cause must therefore be in the nerves of hearing.

The Filtration of Typhoid Germs.—An important letter on this subject, with the general terms of which we entirely agree, has been published in the "Sanitary Record," June 3. Dr. Tripe calls attention to the statements recently made by Mr. Wanklyn, at the Society of Medical Officers of Health, one of which concerned the purification of water supposed to contain the germs of typhoid fever. The following quotation from Dr. Tripe's letter will fully explain his position and that of Mr. Wanklyn:—"I attended the meeting, but arrived too late to hear Mr. Wanklyn's speech; but was informed that he had stated that the non-detection of albuminoid matter, by Nessler's test, after distillation of filtered water with an alkaline permanganate, was strong proof of the absence of typhoid germs. The issue thus taken is one of the greatest importance, because if Mr. Wanklyn's statement be true, the most polluted water can be rendered potable and innocuous by filtration through a moderately thick bed of filtering materials. Certainly this statement is contrary to the opinions held by most other chemists, and if it be based on the absence of ammonia after distillation of the suspected water with an alkaline permanganate, the assumption is almost certainly erroneous, as Mr. Wanklyn himself admits that all the albuminoid matters are not converted into ammonia by his process. He would appear to have made this statement without knowing the size of the minute organisms which are suspected to be typhoid germs, otherwise he could not have placed any reliance on the Nessler test. Dr. Klein says that they are one-third the size of the blood corpuscles of man, but his engravings show them to be much smaller, and to possess only about one-sixth the area of a blood-disc. Now if they are similar to bacteria in their mode and rapidity of increase, it would be only necessary for a very few to obtain admission into the human body to set up their specific action, provided the person were susceptible to their influence. Does Mr. Wanklyn say that he could detect by the albuminoid ammonia process a dozen of these, which are less than the total bulk of one blood corpuscle, in half-a-pint of water? and if not, where is his argument?"

METALLURGY, MINERALOGY, AND MINING.

Compressed Peat.—"Silliman's American Journal" states that peat pressed into blocks and made so compact that a cubic foot weighs 85 to 100 pounds, is manufactured by Mr. A. E. Barthel, of Detroit, Michigan, and sells for one and a half dollars per ton.

The New Metal Gallium.—At a meeting of the French Academy the Secretary opened a sealed note deposited by M. Lecocq de Boisbaudran, the first paragraph of which reads thus:—"Day before yesterday, on Friday, the 27th of August, 1875, between three and four o'clock in the afternoon, I obtained indications of the probable existence of a new simple body among the products of the chemical examination of a blende coming from the mine of Pierrefitte, valley of Argeles, Pyrenees." The evidence relied on to prove this discovery, a part of which evidence was given in the sealed note and another part in a note read at the same meeting, is: (1) the oxide (or perhaps a basic salt) is precipitated slowly by metallic zinc in a solution containing chlorides and sulphates; (2) its salts are easily precipitated by barium carbonate in the cold; and (3) it gives a spectrum showing two violet lines of wave lengths 417 and 404 respectively. In all its other chemical reactions it closely resembles zinc; though in the precipitations it has always the preference when these are incomplete. To the metal thus indicated, Lecocq de Boisbaudran gave the name "Gallium." In a more recent paper he gives additional facts regarding the new metal, which he has been able to free almost entirely from zinc.

METEOROLOGY.

The great Iowa Meteor.—An account of this very large stone is given by the "Boston Journal of Chemistry," in the following terms:—"Some time ago a meteor of extraordinary splendour appeared in the heavens, over the State of Iowa, and after dazzling the eyes of all those who were so fortunate as to see it, burst asunder with a loud report, and in a few seconds disappeared. Fragments of this meteoric mass were scattered over a wide extent of country, and more than 700 pounds have up to the present date been picked up by various persons, and sold to geologists, chemists, and others, at high prices. We are under great obligations to C. W. Irish, Esq., civil engineer, of Iowa City, for a splendid specimen of the stone, which he kindly sent to us last autumn. The specimen weighs about 11 ounces, and accompanying it was a small fragment, which was sent for chemical examination. Our time has been so occupied that we have not been able to bestow upon it analytical labour, but intend shortly to do so. We presume, however, that it does not differ essentially from the specimens examined by Professor Henrichs, and we present in tabular form his results:—

Meteorites	Iron	Nickel	Sulphur	Ferrous Oxide	Magnesia	Lime	Silica	Sum
Non-Magnetic:								
Triolite	1·1	—	0·7	(1·5)	—	—	—	1·8
Hyalosiderite . .	—	—	—	15·2	17·5	0·6	19·6	52·9
Hypersthene . . .	—	—	—	8·8	9·7	2·2	24·2	44·9
Loss, traces . .	—	—	—	—	—	—	—	0·4
Sum	1·1	—	0·7	24·0	27·2	2·8	43·8	100·0
Magnetic:								
Nickeliferous Iron .	6·6	0·9	—	—	—	—	—	7·5
Total	7·7	0·9	0·7	24·0	26·2	2·8	43·8	107·5

The stone, in physical appearance, resembles in most respects those which have fallen upon other parts of our planet, but in chemical composition it varies essentially. It is covered with a black crust, formed during the cosmical part of its motion through the earth's atmosphere. This crust, in the view of Professor Henrichs, is not due to fusion, but simply to the heating of the outer layer of the stone to a red heat. The interior is of a greyish colour, and resembles iron stones of terrestrial origin; when exposed to a red-heat the colour is changed to a black like the crust."

A Chronicle of the recent Falls of Meteorites is given in the "Academy," May 13. On Sept. 14, 1875, at 4 P.M., a meteorite fell at Supino, in the district of Frosinone, Italy, of which Keller publishes a short notice in the "Opinione," Sept. 28, 1875. Its descent was accompanied with the hissing noise and explosion usually observed on such occasions. It is stated that the meteorite took an almost horizontal direction towards a house situated in Supino, which, owing to its having a parapet pierced with apertures, it passed without impact, and that it was then lost to sight. Fragments, respectively weighing 364, 199, 29, and 18·5 grammes, were afterwards found. After the lapse of more than 40 years (if we except the curious explosion which took place over Writtle, near Chelmsford, on Sept. 7, 1875, and that over Bradford, on Sept. 15, 1875, when no meteorites were found) a meteorite appears to have fallen in England on the 20th of last month, at Crudgington, near Wellington, Salop, at 3.40 P.M. It penetrated the earth to the depth of 18 inches, and is stated to weigh $7\frac{3}{4}$ lbs. It was exhibited last week, at a meeting of the Birmingham Natural History Society, when a resolution was passed that the meteorite should become the property of the nation, and be submitted to the fullest scientific investigation.

MICROSCOPY.

The Early History of Microscopy.—Dr. H. A. Hagen has a paper in the "American Naturalist" (March, 1876) which, while dealing with a kindred subject, alludes to this. He says it is well known that magnifying-glasses have been found among the Assyrian relics and the ruins of Pompeii, but the use of their magnifying power is nowhere recorded, though it is probable that some of the admirable gems of the ancients were cut with the help of lenses. Spectacles, perhaps in some way known in Rome, and even used by Nero, are said to have been invented at the end of the thirteenth century in Italy. Magnifying-glasses were manufactured by Arabians, and later by Roger Bacon, but certainly not used for the purposes of natural history before the beginning of the seventeenth century. Italy and Holland dispute the honour of the invention, which was perhaps simultaneous in the two countries. The great advantages of lenses for observation were directly acknowledged, and even augmented, by the invention of the compound microscope. Fontana in Rome and Drebbel in Holland are the rival inventors.

Microscopical Papers for the Quarter.—The following is a list of the several papers relating to microscopy that have appeared in the "Monthly Microscopical Journal" for April, May, and June :—

On a New Arrangement for Illuminating and Centering with High Powers. By Rev. W. H. Dallinger, V.P.R.M.S.—The Identification of Liquid Carbonic Acid in Mineral Cavities. By Walter Noel Hartley, F.C.S. (King's College, London).—On some Structures in Obsidian, Perlite, and Leucite. By Frank Rutley, F.G.S. (H.M. Geological Survey).—On the Aperture of Object-glasses. By F. H. Wenham.—On Zeiss' $\frac{1}{25}$th Immersion. By W. J. Hickie, M.A.—Notes on the

Markings of Navicula rhomboides. By Dr. J. J. Woodward, U. S. Army.—Some Results of a Microscopical Study of the Belgian Plutonic Rocks. By A. Rénard, S. J.—A New Microscopic Slide. By M. Ernest Vanden Broeck.—Measurements of Möller's Diatomaceen-Probe-Platten. By Edward W. Morley, Hudson, Ohio, U. S. A.—On the Markings of the Body-scale of the English Gnat and the American Mosquito. By Dr. J. J. Woodward, U. S. Army.—Notes on Micro-photography. By Surgeon-Major Edward J. Gayer, H.M. Indian Army, now Professor of Surgery, Medical College, Calcutta.—On Renulia Sorbyana. By J. F. Blake, F.G.S.—Remarks on Frustulia Saxonica, Navicula rhomboides, and Navicula crassinervis. By Charles Stodder, U. S. A.—On the Measurement of the Angular Aperture of Object-glasses. By Jabez Hogg, Surgeon to the Royal Westminster Ophthalmic Hospital, F.R.M.S., &c.

PHYSICS.

The Waves as a Motive Power.—Mr. B. Tower, who some time since described his method to an English audience, does not appear to have gone on in furtherance of his discovery. His machine consists in principle of a weight supported on a spring, so that it can oscillate on the spring through a considerable range in a vertical line. The scale of the spring, and consequently the natural period of oscillation of the weight, can be varied at will. When it is so adjusted that it synchronizes with the waves, the oscillations become very violent, and a large amount of power can be obtained from them. In practice, the springs consist of highly-compressed air pressing on the rims of hydro-pneumatic cylinders, and the arrangement is such that the vessel containing the compressed air forms the moving weight. At the meeting referred to Mr. Tower exhibited a design of a machine for working an auxiliary propeller of a sailing ship of 1,800 tons displacement. The moving weight in this case is 200 tons, and he showed by calculation that it would give about 30 horse-power in the long swell met with in the tropical calms, 260 horse-power in average ocean waves, and more than 600 horse-power in a heavy head sea. The space occupied by the machine compares favourably with a steam-engine of the same power. He also exhibited a model of the machine, which recently, in a moderate sea, had yielded power at the rate of 1½ horse-power per ton of moving weight.

Experiments on the Periodic Waves of the Swiss Lakes.—At a recent meeting of the Physical Society of London (May 27), Professor Forel, of Morges, Switzerland, gave, in French, an account of some interesting experiments which he has recently made on the periodic waves which take place on the Swiss lakes, and are there called "Seiches." It was long since observed that the waters of most of these lakes are subject to a more or less regular rise and fall, which at times have been found to be as much as one or two mètres. M. Forel has studied this phenomenon in nine different lakes, and finds that it varies with the length and depth of the lake, and that the waves are in every way analogous to those already studied by Professor Guthrie in artificial troughs, and follow the laws which he has deduced from his experiments. Most of the experiments in Switzerland were made on the Lake of Geneva, but that of Neuchatel was found to be best fitted for the study of the subject, possessing, as it does, an extremely regular geometric form. The apparatus he employed was very sensitive to the motion of the water, being capable of registering the waves caused by a steam-boat half an hour after it had passed, and five minutes before its arrival; and was so constructed as to eliminate the effect of common waves, and to register the motion, side by side, with a record of the state of the barometer, on paper kept in continuous motion. While he found the duration of waves to be ten minutes at Morges it was seventy minutes at Geneva, and this is explained by the narrowness of the neck of the lake at the latter place. This period he proved to be independent of the amplitude, and to be least in the shortest lakes. For shallow lakes the period is lengthened; and his observations show that the period is a function of the length and depth, and that longitudinal and transverse waves may co-exist, just as Professor Guthrie has shown to be the case in troughs.

Imitation Snow Crystals.—M. Dogiel, of St. Petersburg, selects a substance which crystallizes like snow, in a great variety of forms of the hexagonal system. And this substance is iodoform. To show the multiplicity of forms, M. Dogiel dissolves iodoform in boiling (90 per cent.) alcohol, and lets the solution cool in water of different temperatures. He gets mostly tabular crystals, when a solution containing 15 to 30 per cent. of iodoform is kept ten minutes in water of about 14° to 15° C.; whereas star-shaped and often very complicated crystals are had at temperatures of 26° to 37°. Some other modifications of the result are described by M. Dogiel, in a paper recently published, and he also gives drawings of the crystals he obtained.

A Remarkable Atmospheric Phenomenon at Ceylon.—The Rev. R. Abbay sent a communication on this subject to the Physical Society, May 27. In speaking of several of these phenomena he says that the most striking is witnessed from the summit of Adam's Peak, which is a mountain rising extremely abruptly from the low country to an elevation of 7,200 feet above the sea. The phenomenon referred to is seen at sunrise, and consists *apparently* of an elongated shadow of the mountain, projecting westward to a distance of about 70 miles. As the sun rises higher it rapidly approaches the mountain, and appears at the same time to rise above the observer in the form of a gigantic pyramid of shadow. Distant objects may be seen through it, so that it is not really a shadow on the land, but a veil of darkness between the peak and the low country. It continues to rapidly approach and rise until it seems to fall back upon the observer, like a ladder which has been reared beyond the vertical, and the next instant it is gone. Mr. Abbay suggests the following explanation of the phenomenon:—The average temperature at night in the low country during the dry season is between 70° and 80° F., and that at the summit of the peak is 30° or 40° F.; consequently, the low strata of air are much the less dense, and an almost horizontal ray of light passing over the summit must be refracted upwards and suffer total internal reflection, as in an ordinary mirage. On this supposition the veil must become more and more vertical as the rays fall less horizontally, and this will continue until they reach the critical angle, when total internal reflection ceases, and it suddenly disappears. Its apparent tilting over on the spectator is probably an illusion, produced by the rapid approach and the rising of the dark veil without any gradual disappearance which can be watched and estimated. It will be evident that the illumination of the innumerable particles floating in the atmosphere causes the aerial shadow to be visible by contrast. Another interesting phenomenon visible in the mountain districts admits of an equally simple explanation. At times broad beams, apparently of bluish light, may be seen extending from the zenith downwards, converging as they approach the horizon. The spaces between them have the ordinary illumination of the rest of the sky. If we suppose, as is frequently the case, that the lower strata of air are *colder* than the upper, the reflection spoken of in the case of Adam's Peak will be downwards instead of upwards. If several isolated masses of clouds partially obscure the sun, we may have several corresponding inverted veils of darkness, like blue rays in the sky, all apparently converging towards the same point below the horizon. This latter phenomena is called by the natives "Buddha's rays."

A Simple Form of Heliostat.—The heliostat is such an useful instrument, not merely for the physicist, but for the photographer, that any mode of improving it is of interest. At the meeting of the Physical Society on April 29, the Secretary read a communication from Sir John Conroy, Bart., "On a Simple Form of Heliostat." The defect of Fahrenheit's heliostat, in which the beam of sunlight is reflected by a mirror moved by clockwork in a direction parallel to the axis of the earth, and then in the required direction by a fixed mirror, consists in the great loss of light. The author substitutes two silvered mirrors for the looking-glasses usually employed, and he has shown that the loss of light with this arrangement is less than when the light is once reflected from a looking-glass.

Electric Communications without Wires.—It would seem from recent experiments that it is perfectly possible to convey a message for a certain distance along the earth without any conducting wire whatever. But M. Th. Du Moncel has explained to the French Academy (May 8) that the idea of communication without wires is far from novel, having been experimentally tested thirty years ago, both in England and America. Thus, messages were sent from Gosport to Portsmouth (and, we believe, across to the Isle of Wight), a distance of about 3 kilometres.

Ice-making Machines —Professor Hoffmann has the following account in the "Chemical News," May 12, of a novel form of ice-machine. He says that since the beginning of 1873 Nehrlich & Co., of Frankfort-on-the-Main, make the Windhausen machine with two cylinders of one size only, with especial regard to the demand in breweries. It requires a 40-horse power engine, and is guaranteed to yield hourly 2,500 cubic metres of air at temperatures of from $-30°$ to $-50°$. If we assume that these temperatures refer to initial temperatures of from 10° to 30°, the total reduction of temperature amounts to 60°, whence the amount of the negative heat units may be calculated as 50,000, corresponding at most to 400 kilos. of ice. If the production of ice were the object in view, the same quantity of air might be made to circulate. A steam-engine of 40-horse power consumes hourly 80 kilos. of coal; consequently 1 kilo. of coal would give 5 kilos. of ice—a very favourable result. Such a machine, including the engine, cost in 1873 66,000 marks (3,300*l.*) The sum certainly seems immense.

ZOOLOGY AND COMPARATIVE ANATOMY.

The "Challenger" Expedition.—Since our last issue this expedition has come to a conclusion. Much of the work, however, remains to be published, and will occupy the different members of the staff for the next six or nine months. All that has been published concerning her trip around the world will be found in the long and important paper which was published in an earlier number by W. F. Galton, F.R.C.S.

Snakes that Eat Snakes.—One of these creatures, which is now at the gardens of the Zoological Society, has, during its stay in this climate, devoured an enormous number of the common English snake. We learn from an American contemporary that some years ago Professor Cope described the snake-eating habits of the *Oxyrrhopus plumbeus* (Wied), a rather large species of snake which is abundant in the intertropical parts of America. A specimen of it from Martinique was observed to have swallowed the greater part of a large *fer-de-lance*, the largest venomous snake in the West Indies. The *Oxyrrhopus* had seized the *fer-de-lance* by the snout, thus preventing it from inflicting fatal wounds, and had swallowed a greater part of its length, when caught and preserved by the collector. More recently a specimen was brought by Mr. Gabb from Costa Rica, almost five feet in length, which had swallowed nearly three feet of a large harmless snake (*Herpetrodryas carinatus*) about six feet in length. The head was partially digested, while three feet projected from the mouth of the *Oxyrrhopus* in a sound condition. The *Oxyrrhopus* is entirely harmless, although spirited and pugnacious in its manners. Professor Cope suggests that its introduction into regions infested with venomous snakes, like the island of Martinique, would be followed by beneficial results. The East Indian snake-eater, *Naja elaps*, is unavailable for this purpose, as it is itself one of the most dangerous of venomous snakes.

Singular Custom adopted by a Tree-Frog.—Professor Peters has lately described the mode of deposit of its eggs employed by a species of tree-frog (*Polypedates*) from tropical Western Africa. This species deposits its eggs, as is usual among batrachians, in a mass of albuminous jelly; but instead of placing this in the water, it attaches it to the leaves of trees which border the shore and overhang a water-hole or pond. Here the albumen speedily dries, forming a horny or glazed coating of the leaf, inclosing the unimpregnated eggs in a strong envelope. Upon the advent of the rainy season, the albumen is softened, and with the eggs is washed into the pool below, now filled with water. Here the male frog finds the masses, and occupies himself with their impregnation.

Crustacea of the North Pacific Exploring Expedition.—It was supposed that the papers of Dr. Stimpson describing these crustacea had been destroyed.

However, the "American Naturalist" tells us that a careful examination of the papers left at the Smithsonian Institution by the late Dr. Stimpson has revealed the existence of the complete MSS. of his final report on the Crustacea of the North Pacific Exploring Expedition as far as the end of the Anomoura, with beautiful figures of one hundred and thirty-seven of the new species. It was supposed that these had perished with Dr. Stimpson's other MSS., and with the collections they described, in the great Chicago fire. It hopes they will soon be published.

A New Tænia from Rhea Americana.—At a recent meeting of the Philadelphia Academy of Sciences, Dr. Chapman called the attention of the members to a new species of tænia which he had found in the alimentary canal of the *Rhea Americana.* According to Diesing there exists in the Struthio a tænia, but as no description is given he could not say whether the species are the same. It is very probable, however, that they are so. If future investigation should show this to be correct, it will offer another illustration of closely related forms having the same entozoa. The tænia from the *Rhea* varies from nine to ten inches in length. Its head measures $\frac{1}{33}$ of an inch in breadth and $\frac{1}{25}$ of an inch in length (to beginning of first segment). The head is provided with four suckers. The cervical segments are rounded off at the articulations, but the mature ones are serrated. The genital aperture is lateral, and alternates from side to side. Sometimes there will be as many as five successive segments on one side exhibiting these apertures, and then five will be seen on the opposite side of the next five successive segments. The penis could be protruded by compression, and the vagina readily seen. From the fact of the head being rather thickly set upon this species, the name *Tænia tauricollis* was proposed for it.

The Mammals of the Assyrian Sculptures.—The Rev. W. Houghton, who is a well-known contributor to this journal, recently read a paper on the above subject before the Society of Biblical Archæology (May 2, 1876). Beginning with the order Quadrumana, Mr. Houghton said two species were represented. He referred to the absurdly human appearance of the monkeys of the sculptures : the face is that of a man with a fringe of whiskers round it neatly trimmed, but one figure more true to nature indicates the species of monkey—viz., *Presbyter entellus*, the Hoonuman of India, or some closely allied species. There was also another species, the *Macacus Silenus.* The Assyrian word for monkey was *u-du-mu*, the same as the Hebrew word *Adam*, "a man;" compare our "anthropoid ape." Of the order *Feræ* there are mentioned the lion, the hyena (in Accadian *Lig-bar-ra*, "striped dog"); the bear, *Ursus syriacus*, especially as being of various colours, and the leopard. Other wild animals were the hare, *Lepus sinaiticus* (*ka-zin-na*, "face of the desert"); the wild bull, which was clearly a *Bos* and not a *Bubalus*, most probably *Bos primigenius* of the tertiary period; the wild goat (*Capra sinaitica*), the Asiatic steinboc or ibex; the wild sheep (*Caprovis orientalis*), the wild deer (*Cervus mesopotamicus*), and other species, *Cervus elaphus* and *Cervus Maral*, or Persian deer; the gazelle (*G. Dorcas*); the wild ass (*Equus hemippus*); the elephant (*Elephas indicus*); the rhinoceros, or, as it is called on the black obelisk of Shalmaneser, "the ox from the river Saceya;" and the wild boar (*Sus scrofa*).

The Editor's Farewell.

HAVING occupied the editorial chair for thirteen years, during which we sought to raise the standard of the journal, and to avoid as much as possible the merely popular element, we now bid our readers farewell.

It is the intention of the publisher to make the journal a more popular one than it has been. Indeed he endeavours to reverse the position we have attempted to give the periodical, and possibly with better financial results.

This, then, being his view of the matter, it becomes us only to take our leave, and to wish the journal every success under its new management.

And in bidding adieu to past contributors, who have invariably extended every effort of their pens toward the successful cultivation of their several subjects, we trust that they will overlook anything that may have seemed on our part like inconsiderateness.

If they will only remember that an editorial chair is not a seat which is remarkable for its ease and comfort, they will readily make allowance for its past occupant.

We have, we think, been fair to all sides, whilst we have given the most prominent position to the views of Darwin, Huxley, Lyell, Wallace, Hooker, &c.—to the men to whom modern science is most indebted for all its progress. If we have been unjust in our wielding of the editorial pen, we know it not; and of all who may consider that we have been, we humbly ask their pardon. And now we say, Good-bye, Vale, Farewell.

PRACTICAL NOTES ON "HETEROGENESIS,"

A REPUTED FEATURE OF SPONTANEOUS GENERATION.

By the Rev. W. H. DALLINGER, V.P.R.M.S.

[PLATES CXXXIX. AND CXL.]

THE doctrine of "spontaneous generation" is declared by its principal advocate * to involve not merely the origin of living forms from not-living elements, but also the origination from living beings, more or less complex in organisation, of other living units wholly different from themselves, and having no tendency to assume or revert to the parental type. This means briefly that one organised form may, by the operation of some occult laws, produce another organised form wholly unlike itself, and which may be not only of a different genus, but of a different order of a different class—nay, probably altogether of a different kingdom. This is an assertion of caprice in biological laws. Their action is uncertain. True, these remarkable phenomena are not at present asserted of the more easily accessible and highly developed organisms; but it may be instructive to note that if they were so applicable it would admit, for example, of a humming-bird being hatched from a snake's egg, or a gorilla being born from a kangaroo; for neither of these instances is more startling than the alleged transformation of Euglenæ and Chlorococcus directly into—rotifers! Yet not only this, but many other things equally as remarkable, are sanctioned and asserted by Dr. Bastian; and the credence of biologists is asked for these affirmations, with as little apology or hesitation, as for the fact that a tadpole is the precursor of a frog, or a chrysalis of a butterfly.

This of course gives much greater complexity to the hypothesis of spontaneous generation; but at the same time it gives it character. It is at least unique.

* "The Beginnings of Life." Dr. H. C. Bastian. Vol. i. p. 244.

Plate CXXXIX

W.H. Dallinger del. ad nat. W. West & Co lith.

Illustrations of mistaken "Heterogenesis"

The former part of the hypothesis—that living forms originate in not-living elements—arise, *de novo*, from dead matter—has received, and is still receiving, the most careful and persistent consideration of biologists; and the present balance of facts may safely be accepted from the pen of Professor Huxley, who, in a most comprehensive and elaborate article on "Biology," in the edition of "Encyclopædia Britannica," now passing through the press, affirms that "The biological sciences are sharply marked off from the abiological, or those which treat of the phenomena manifested by not-living matter, in so far as the properties of living matter distinguish it absolutely from all other kinds of things, and as the present state of knowledge furnishes us with no link between the living and the not-living." *

But the latter part of the hypothesis with which we are now dealing—that of the production of one kind of organism by another of an altogether different nature—although it has had some irresistible refutations, has not been seriously considered by the majority of leading biologists. The reason is plain. It is absolutely unsustained by facts. It is based on careless and incompetent observation, or exaggerated inference. It is out of harmony with all the most valuable observations of the most careful observers, and contradicts all we have otherwise learned of nature's methods. Its refutation, so far as it need be accomplished, may be safely relegated to specialists to be dealt with in detail, and this in more than one instance has been already done with remarkable effect. The philosophical biologist can afford to discard it; it does not require his serious consideration; his experience has taught him its fallacy; whilst its influence upon the student can neither be great nor lasting. To every mind, indeed, it should be apparent that, before such remarkable affirmations are presented as scientific *facts*, every pains should have been taken to make them such. These extraordinary "transformations," which are not only alleged, but plentifully figured, ought surely to have been so scrutinised, repeated, controlled, and purged of all fallacy, as to make the recorded phenomena at least as certain as the manner of recording them would lead the reader to infer it was, and as the seriousness of the issue demanded. But that this has not been the case is painfully apparent. In the instances of reputed "transformation" which are the result of Dr. Bastian's personal investigation, there is a looseness of method, and a disregard of detail, minutiæ, and above all, continuity of research, which stands in singular contrast to the precision and persistence of modern science, even in the simplest matters. But the surprise which this awakens is enhanced by the fact that

* Vol. iii. p. 679, 9th Ed. 1875.

he gives without question, nay, with implicit sanction, a series of "heterogenetic" phenomena from "the much neglected memoir of Dr. Gros" and others. The former has been for twenty years before the scientific world, during which time unparalleled vigour has been displayed by biologists in every department of research, and especially in the development of minute life-forms; which have been studied with constantly improving apparatus. But during the whole of this time not a single instance of accepted corroboration of these strange transformations can be pointed out. But thousands of observations have been made and recorded that are directly adverse to the whole. Surely this alone should have suggested caution, and the careful and competent repetition of both Gros' and his own observations. But it was not so.

All who have any practical knowledge of the nature of such enquiries will admit that an indispensable condition—a *sine quâ non*—to accurate results, is *continuity of observation*; and that, moreover, upon the same organisms, and with the very best appliances; all of which should be guarded by the elimination of every conceivable source of fallacy.

Discontinuous or interrupted observation is, in such inquiries, worse than useless. It is at once a prolific and a fascinating source of error. The same must be said of not working out the history of the *same individual.* In all such researches scores, nay, hundreds of hours are wasted—or rather apparently lost—in following the organism to a certain point, and then it dies; or some accident happens, or some distraction to the observer arises; and the temptation is to take up the observation again *upon another form in apparently the same developmental condition as the former one was in when the interruption happened.* No source of error can be more serious in practice, when the objects of research are so minute and unknown. Hundreds of instances of this might be given. Indeed there can be no accuracy unless the observation begins again *ab initio*, and is carried persistently to the end.

Let any one take a moderately decomposed infusion of fish or brain, and put a small clear drop of it from the point of a fine "dipper into half a wine-glass of Cohn's nutritive fluid, and leave it for a few hours. Let a drop of this be put on in the usual way upon the continuous moist stage described in the "Researches into the Life-History of the Monads," and let a $\frac{1}{16}$ in. objective be used. The probability is that for two days nothing will be visible to the most careful scrutiny but bacteria—at least it can be so arranged. But now, the observer who spends two or three hours discontinuously at the instrument will probably observe that what *look like* some of the bacteria are getting larger; and if then a night should intervene, he will, on returning to his instrument in the morning, be startled by discovering that an anchored springing monad is dotted over the "field," with perhaps another form swimming freely about. Now in this case the inference would be more legitimate than in the majority of Dr. Bastian's that the bacteria had been "transformed" into monads. There were the bacteria, alone—some of them were seen to enlarge—and here in the course of a few hours are vigorous and distinctive monads! *Post hoc, propter hoc.* But let this group of monads be now casually examined in the same way: in the course of some hours many of these will become very still and sac-like; the crowding of the field will increase; the difficulty of discrimination to interrupted observation will become greater; and at the end of twelve hours more, some splendid specimens of *Kerona pustulata* will be asserting their mastery of the field. Now there is no instance presented by Dr. Bastian, as a case of "transformation," that has a more exact basis to stand upon than is presented here for inferring that the bacteria were transformed into monads, and that these monads preferred to perpetuate existence as Kerona. But what are the facts? The apparently fattening or enlarging bacteria were simply developing monad germs, and if *continuously* watched all the steps from the germ to the adult form would have been seen; and the same continuity of observation would have shown that the Kerona had a genetic origin as independent of the monads as the monads had of the bacteria. Indeed, low in the scale of organisation as the monads are, I do not hesitate to affirm that in their development they give no sanction or shadow of support to the hypothesis of "heterogenesis." "On the contrary, the life-cycle of a monad is as rigidly circumscribed within defined limits as that of a mollusc or a bird. There is no indication of any unusual or more intense method of specific mutation than those resulting from the secular processes involved in the Darwinian law, which is held to furnish the only legitimate theory of the origin of species." *

But to the young and ardent observer the example set by Dr. Bastian might prove the utmost evil. It is possible to infer almost anything from discontinuous observation. The simplest cases illustrate this. Only recently a friend, an ardent microscopist, announced to me his discovery of a perfect demonstration of "heterogenesis." We had some days before made an accidental gathering of the finest specimens of *Volvox globator* which I had ever seen. These were placed in a small clean trough, with clear water. With 100 diameters nothing of moment was visible but the volvoxes. The trough containing them was put

* "Further Researches into the Life-History of the Monads." Dallinger and Drysdale. 'M. M. J.' vol. xiii. p. 189.

into a moist chamber, to prevent evaporation, and left for four days. Quite by accident it was examined again, when to the surprise and extacy of my friend there was nothing living in the trough but rotifers. The volvoxes were all dead, and lay at the bottom of the trough, and the rotifers were voraciously eating them. More than that, in two of the dead volvoxes the form of a similar rotifer could be distinctly seen imbedded. My attention was called to the fact as a "transformation." I at once saw that this was a case, constantly re-appearing, of the rotifer parasitic within the volvox; and as I had never seen this, or the rotifer itself, before, I made a drawing of the whole field of dead volvoxes with rotifers devouring them, as seen with 12 diameters, and given at fig. 1, Pl. CXXXIX., and also made a careful drawing of the rotifer alone under 80 diameters, which is given in fig. 2. I remembered that Ehrenberg had observed at least two such rotifers, and on referring, found that my drawing came nearest to his *Notommata parasitica.* There are minute differences between the form I saw and Ehrenberg's, and there is a discrepancy in size; but there can be little doubt that it is either this form or a variety. I was now induced to make another gathering; and within the cells of four out of some thousands of volvoxes I saw the rotifer moving with sudden fierce jerks, devouring the young and eventually bursting the parent cell and escaping. A drawing of the volvox with its parasitic rotifer is seen in fig. 3, where the former is slightly out of focus that the latter might be clearly perceived.

Of course no practical naturalist would have been for a moment mistaken here. But a want of knowledge of all the facts, and a bias to a certain theory, could easily conclude that this was a case of volvoxes being "transmuted" into rotifers. And the same kind of inference is a possible danger to any observer with a bias, when the objects are comparatively inaccessible, or their life-histories unknown. But in the instance given, would it not be equally just to infer that the *Trichina spiralis* in a man or a pig was "transmuted" muscle? or that *Toenia solium* was—say the "transmuted" villi of the digestive tract?

But a case yet more instructive presents itself. In the June of 1874, a friend at that time residing at Sandhurst, in Berkshire, sent me a large bottle of water from a pond there, containing a remarkable monad, with two trailing flagella and a swiftly lashing anterior one. The form was quite new to me, and is seen in fig. 4. I did all in my power to keep it alive, that I might if possible work out its history when some work then claiming all my time should be finished. The water containing the monads was placed in circumstances that would as far as possible prevent evaporation and admit air. But they rapidly diminished in number until the middle of August, and then remained stationary until the early part of September was past, and then there appeared suddenly, a minute, and to me entirely unknown rotifer. It was one of remarkable form and deportment, which I at once sketched, and have reproduced at fig. 5. I now examined with great care, and found the monad but feebly present; indeed only a very few were in the normal condition and shape, whilst a considerable number were slowly moving in the shape drawn at fig. 6. Now this was grotesque in the highest degree; for the newly imported rotifer had decidedly a portrait of its own when seen in profile, as fig. 7 will testify; and the modified monad approximated to this in a simply ludicrous manner. The absurd caricature seen in fig. 6 was enhanced by amœboid elongations and sharpenings of the lower part of the body, and by the protrusion of pseudopodal spines at *a*, *b*, and *c*; which still further pointed to the hypothesis that this simple creature, by virtue of the "laws" of "heterogenesis," was aiming at a higher sphere. Certainly, to a mind that could see its way to "heterogenesis," this was a suggestive instance, and might have been fairly employed (on the pattern already presented) to swell the instances of the "transmutation of monads into rotifers."

But a further acquaintance with the monad wholly dispels this dream: it was merely passing honestly through a phase in its life-history, after the fashion of its ancestors. And as to the rotifer, my attention was afterwards called to the fact that Mr. Gosse had seen and figured it; and in his account of it, I find a most instructive passage.* He names it *Dinocharis Collinsii*, and tells us that a bottle of the water in which this rotifer was found was taken away by a friend; but although it was well searched the rotifer was not found, nor indeed was "anything of interest" discovered. It was nevertheless retained; and after having been kept for more than four months it was suddenly and in an unexpected manner seen to be "swarming with these interesting creatures"—an event extremely similar to the one I record; only, in this latter case, there were no monads beforehand to suffer "transmutation."

Now whoever has carefully read the reputed facts for "heterogenesis" will be fain to admit that there are very few of them that offer more reasonable ground for the inference made than exists in this instance. Discontinuous observation, aided by imagination, sees the monad, then the form of a monad mid-way between itself and the rotifer, and finally the latter; and, after its fashion, the case would be established. That this is no exaggeration of the kind of reasoning employed, we may fairly test by the facts presented.

* "Intellectual Observer," vol. x. pp. 270-1.

Most microscopists have at some time made acquaintance with the water-bears of our ponds, and a good many have followed their development. Whoever has done the latter has fully convinced himself of the truth of the statements of Kölliker, Frey, Doyère, Kaufmann and others, that the tardigrades in every instance produce large fecundated eggs, from which young, closely resembling the parents, emerge. Another feature of the tardigrades is the extreme hardness and toughness of their " skin." It is in point of fact, speaking relatively to the Arthoropoda, almost a " shell." This skin it is also well known is " cast " by the creature, and it forms, in the case of the female, a shelter or protection for her eggs.

Now Dr. Bastian tells us that the power of reproduction in these forms is not limited to the " rudimentary generative organ," because " Dr. Gros tells us that the dead tardigrades may ultimately be resolved into specimens of Actinophrys, Peranemata, or Arcellinæ," and that these products may at different times be either all of one kind, or intermixed with each other and with young tardigrades! On the strength of this discovery we are presented with a drawing which I reproduce, fig. 8. The subscription which accompanies this is very suggestive. It runs thus, viz.: " Seven large germs *into which the total internal substance of the parent has become resolved*, each of them being *capable of developing into a tardigrade*."

Now, wherever there are plenty of tardigrades there will be found dead forms, with their internal structure unchanged, and others which are mere empty shells or skins. Some of these latter are, doubtless, " cast skins ; " but the dead water-bears, in a trough not very plentifully supplied with food, will soon be attacked by *paramecia* ; and although the aperture they make may not be clearly seen, they somehow get *into* the body of the animals, and gradually devour all that is in it ; and after cleaning it as thoroughly as ants will a small skeleton, leave it a hollow but perfect form. It is now open to the chapter of accidents, and it can be no matter for surprise that the minute eggs of aquatic creatures enter it and hatch there.

This may be easily illustrated. Mr. G. F. Chantrell, the Secretary of the Microscopical Society of Liverpool, is a very careful and constant observer of pond-life. He has endeavoured to verify or substantiate some of the more marked cases presented by Dr. Bastian. But his method of examination is, of necessity, an interrupted one. He has frequently called my attention to curious cases of apparent " transmutation ; " and I have before me now some of his drawings of these, taken from nature. In fig. 9 I reproduce one, which it will be seen is extremely like the one figured by Bastian (fig. 8), which, it must be remembered, he affirms, on the assurance of Gros, was full of germs by the *resolution* of its internal substance; and that each of the germs was "capable of developing into a tardigrade." But, fortunately, Mr. Chantrell did not leave the germs to their capabilities; he suspiciously followed them out, and they became *Stentor Caeruleus*! As drawn after hatching, they are presented in the attached or fixed state at 9*a*, and in the free swimming condition at 9*b*. Clearly the eggs of the stentor had got into the dead hollow body of the tardigrade, and developed there!

That this inference is a correct one I have repeatedly verified, and at fig. 10 give an additional instance in proof. This is the hollow, perfectly transparent skin of a tardigrade. Nothing has been left within but the hard retractile tube and "gizzard," and these, as seen at *a*, have fallen from their true position. At *b* a small oval body was seen, perfectly, and watched; and eventually the small rotifer *c*—probably *Monura dulcis*—emerged from it, and at length escaped from the skin of the tardigrade altogether.

Surely it is unsatisfactory science to consider a phenomenon like this "heterogenesis," and to label it "homogenetic pangenesis in tardigrades!"

The "transmutations" of the living protoplasm of vegetables, given in evidence of the hypothesis, are all subject to the same defect. That is everywhere *assumed*, which at least *might* have presented another explanation, had all the possibilities of error been eliminated, and continuous observations been made. I select, to illustrate this, fig. 11, Pl. CXL., from Mr. Chantrell's drawings. This object I saw in the living state. It is described as a "curious fungoid growth, found in a trough with Anacharis, some weeks old." Now it appeared to me, in the living state, to be quite impossible, apart from unbroken observation, to determine whether the amœbæ or amœboid masses, *a a*, attached to the arms of the fungus, were anything more than preying upon it and devouring it as food. But the presumption was that the protoplasm of the fungus was being "transformed" into amœbæ. But can such an inference be maintained? We know how easily the minute spore of the smaller creatures, as, for example, the monads, may penetrate into the very substance of growing vegetable forms, and become inhabitants of the cells. Look, for instance, at the fact of ditomaceæ being absorbed from infusorial soil into the roots of plants, and being built up unaltered into their stem substance.* And that spore may germinate in the cells of plants is fully attested. Besides, what can be more uncertain than the organism to which an amœboid condition of protoplasm belongs? It

* "Silliman's American Journal," May 1876.

might, indeed, be merely a phase in the life development of the "curious fungoid growth" itself. All these possibilities, and a hundred others, in every instance should be most crucially considered before the semblance of a case for "heterogenesis" could even be presented in a strictly scientific form.

But Dr. Bastian has neglected such precautions, and has adopted the observations of others, which are even more invalid. How serious the resulting errors are, in only one direction, has been plainly shown by Professor H. L. Smith,* of America, whose competence to write critically on the subject of diatomaceæ will not be disputed. He has given an absolutely destructive detailed criticism of every important instance of the reputed transmutation of something else into diatoms which Dr. Bastian has presented, and brings out clearly the mistake of attempting to infer the "heterogenetic" origin of vital forms of whose ascertained history the observer was ignorant. Professor Smith says: "I have probably witnessed more of the phenomena of conjugation and growth than any other person, and can affirm, without fear of being disproved, that . . . . any kind of transformation of Pediastreæ or Desmids into Diatoms never has happened—nay more, never will happen." "I look," he continues, "more particulary to the evolution of diatoms, fully convinced, however, that the errors of misinterpreting what he (Dr. Bastian) saw are quite as great with the desmids as with the diatoms." For example, fig. 12, Pl. CXXXIX., is a reproduction of one of Dr. Bastian's figures, which he declares represents, at *e*, *e′*, the "heterogenetic" origin of diatoms from the *Cladaphora* filament A. Professor Smith says, poor as the cut is, we easily recognise the "pedunculated diatoms" as "*Acanthes exilis in its normal condition!*" In fact, it constantly grows naturally thus on *Cladophora*, *Vaucheria* and other algæ. But because Dr. Bastian *was not aware of this*, he took the observation as a fine illustration—which in view of the facts we have no objection to admit—of heterogenesis. Whereas, "if it had been allowed to live it would have continued the process of self-division until finally . . . . a new sporangium would have formed the commencement of a new series."

Again, Dr. Bastian affirms the small forms figured at *l l′* (fig. 12) are algoid vesicles budded off from Vaucheria, and that they "gradually become converted into different kinds of diatoms." And further, "These bodies increased in size, and it soon became obvious that they were young *Naviculæ* (*l l′*); the exact pattern assumed in the early stages is subject to much variation,

* *Vide* "Archebiosis and Heterogenesis," the "Lens," Jan. 1873; and "Quarterly Journ. Micro. Science," vol. xiii. p. 357; and note by Mr. Archer, *ibid.* p. 313.

and several different diatoms *seemed* to be produced corresponding to these initial forms, *m m'*" (fig. 12). "This," says Professor Smith, "would be wonderful if true; but not only is there no evidence that actual diatoms *did* come from the vesicles of *Vaucheria*, but any one familiar with the observation of living Diatoms can tell *where* they did come from. . . . . They were in the gathering . . . . and made their appearance out of the *débris* . . . . as we know they will do under the influence of light. . . . . But, besides, *Diatoms do not grow by increase of size;* there are no such things as broods of young frustules. . . . . The late Dr. Greville . . . . fully agreed with me in this."

"Further, fig. 13, Pl. CXL., is a copy of another illustration given in the "Beginnings of Life." It is declared to represent the "resolution of Euglena into diatoms." It is said concerning it that "the whole of the contents of an euglena *seemed* to have been resolved into distinctly striated *Naviculæ* . . . . *although the earlier stages of the transformation were not seen* (*!*) I have *no doubt* that the diatoms originated in this way." Upon this Professor Smith observes: "He (Dr. Bastian) is more easily satisfied that a euglena can transform into a diatom, which possesses a wonderful silicious and beautifully-sculptured epiderm, than he is that bacteria come from air-germs;" and then he clearly shows that the group of Naviculæ seen in fig. 13 are simply a group that were devoured, and their protoplasm digested, by an amœba. They constantly are ejected in this way from the body of the amœba after the nutrition has been abstracted, and look like an encysted mass with an envelope complete; and even when treated with acids, although the envelope disappears, the frustules still adhere. And Professor Smith has "slides as well as material showing this in abundance."

All this, it may be presumed, is capable of suggesting two things: 1. The danger of attempting to discover *new* modes of "genesis" until we have made ourselves acquainted with the old ones; and 2. That "heterogenesis" is not even a scientific *hypothesis*, for the "facts" on which it is founded have not received scientific investigation.

But hitherto I have dealt with the question as a whole. I may now touch a point with which I have endeavoured to make myself specially familiar. Dr. Bastian claims that bacteria are constantly being "transformed" into *monads*. This, it is affirmed, takes place in what is known as the "proligerous pellicle," or scum which very rapidly forms on the surface of infusions. This pellicle is formed chiefly of bacteria; but it *may* contain every variety of form possible to a given infusion, in the earlier stages of their development. It is worthy of remark that there is not a single instance throughout in which Dr.

Bastian furnishes a correct illustration of a true pellicle. It is always drawn too discreet and too uniform. The bacteria are always matted together in the closest manner, and millions of the minute germs of diverse lowly life-forms might be interspersed in that portion of the pellicle which constitutes the "field" of even the highest powers; and I say advisedly that no microscopist in the world could ever distinguish them.

It is now established that the monads produce germs or spore —and they are extremely minute. If such germs should be interspersed in a given pellicle, their earliest development could not be seen at all; and when it had reached a certain stage it would only be visible under proper and definite conditions. But when the developing monad germ had reached say the size of a bacterium in the pellicle, I presume that Dr. Bastian would not claim any ability to distinguish the one from the other. Nevertheless fig. 14 is the copy of a drawing accepted by him from Pouchet as displaying the origin of "*Monas lens*." *a* is supposed to represent the pellicle, and the small aggregations in it are taken as the initial stage of the "transformation" of these organisms into monads; this is said to go on, until a stage like that seen in *b* is reached; and eventually a flagellated and complete form results, as seen at *c*. And this is presented as a case of heterogenesis. Now the fact is that the monad which is meant to be depicted here (but which is badly drawn) arises in a germ—quite invisible unless looked for under special conditions, and not likely to be seen, with the appliances used, *in a pellicle.* But when this and other monad germs develope, as is constantly the case, in the pellicle, its growth and expansion, and probably other causes, modify, more or less plainly, the immediately surrounding portions of the pellicle, giving it an appearance similar to, but too strongly depicted at, *a* (fig. 14), the point indeed in which the bacteria are supposed to be transforming into monads. After this the germ rapidly progresses, the flagellum is acquired, and the perfect monad swims away. In fact it is no more a case of "heterogenesis" than is the birth of a humming-bird or an ox. And this explanation will apply in every instance; the said "transformations" are slightly altered conditions of the pellicle, resulting from the natural growth of interspersed monad germs.

But nowhere has the egregious mistake of this observer's method so much impressed me as in the illustration on p. 220 of vol. ii. of his "Beginnings of Life." I reproduce as much of it as is required in fig. 15; *a a* are monads which are supposed to have originated in the bacteria. At *c c c* these monads are ("heterogenetically") changing, or changed into *amœbæ.* At *d d* these amœbæ are seen minus the flagella, in an active and a still condition. At *e*, *f*, *g* they have become encysted;

and at *k*, *l*, *m* they are seen to resolve themselves into bacteria again!

Now, if Dr. Bastian had in this case only watched more *continuously*, and with the care required, he would have found that he had here the *complete life-cycle of a monad*; and by doing so would have handed over a useful fact to science. But as it is, the truth is absolutely obscured.

The facts may be briefly glanced at. A monad life-history, in all essential respects agreeing with the "heterogenetic" stages just given, was worked out and published in the "Monthly Microscopical Journal."* It originates in a definitely discovered and carefully followed germ. In fig. 16 is a drawing of a germ arising in the pellicle. This was under a magnification of 3,500 diams.; and in the drawing all is reduced to one-fourth, except the germ just commencing to develop, which is exaggerated, even at the full magnification, for the sake of clearness, and shown at *a*. Fig. 16_2, a drawing made on the same conditions, shows the early development of the germ. Fig. 17 is a fully developed monad. After a greater or less length of time spent in multiplication by fission, it becomes *amœboid*, as seen in fig. 18. This may be compared with *c c c*, fig. 15. This amœboid state becomes very general, and two meet, as at fig. 19, and instantly unite, the blending going on until the two forms are united into one, as seen in fig. 20, each of which may be compared with *d* (lower figure), fig. 15. Encystment now rapidly ensues, and is in progress at fig. 21, which may be put beside *d* (upper figure), fig. 15; and this is complete in fig. 22, perfectly comparable to *e*, *f*, *g*, fig. 15. This cyst eventually pours out—certainly not bacteria—but germs, which were watched continuously into the parent form. Thus what Dr. Bastian supposed was the "transformation" by "heterogenesis" of one vital form into another, was in fact only a series of stages in the metamorphosis through which a monad with an ascertainable history was passing. While in the same journal† another such history is given in which the organism, after passing through similar preceding changes, becomes a cyst, which pours out *living young*.

Thus, wherever certain knowledge is brought to bear upon the reputed cases of "heterogenesis," they are easily shown to be erroneous and misleading, and afford no foundation for the superstructure that has been raised upon them. As no sound philosophy can be opposed to the conception of a continuity in Nature, and no true science can object to its discovery, but must

* "Researches on the Life-History of a Cercomonad." By Dallinger and Drysdale. Vol. x. p. 53.

† Vol. xi. p. 7.

rejoice in it, so no true biologist would reject "heterogenesis" for its own sake, or because it opposed what seemed to be established facts; but as it would be retarding the development of the most sacred of all things—Truth—to accept as evidence of Nature's continuity between the living and the dead Dr. Bastian's proffered evidences—even the latest—of "abiogenesis," so it would be weakness without a name to accept "heterogenesis" on the erroneous facts and equally erroneous inferences of its principal if not its only advocate.

EXPLANATION OF PLATES CXXXIX. AND CXL.

Fig. 1. A group of minute rotifers, which, their history being unknown, might have been supposed to have been transformed *volvoxes*.

Fig. 2. The above rotifer magnified 80 diams., and identified with *Notommata parasita* (Ehr.).

Fig. 3. The same rotifer as a parasite within the volvox, the latter being slightly out of focus to sharpen the image of the rotifer.

Fig. 4. A new and unknown monad, found in a pond at Sandhurst.

Fig. 5. *Dinocharis Collinsii*, which suddenly appeared with the disappearance of the monad (fig. 4).

Fig. 6. A metamorphosis of fig. 4.

Fig. 7. Profile of fig. 5, showing a grotesque similarity between figs. 6 and 7.

Fig. 8. The body of a tardigrade, said by Dr. Bastian to have resolved its "total internal substance" into germs which are said to be capable of development into tardigrades.

Fig. 9. A similar phenomenon seen by Mr. G. F. Chantrell, in which the germs were followed out and became stentors. 9*a*, 9*b*.

Fig. 10. The hollow skin of a tardigrade, showing that it may become the accidental resting and hatching place of small eggs or spore; *b* an egg, and *c* a rotifer (*Monura dulcis*), emerged from it.

Fig. 11. Amœba at *a a*, supposed to have arisen from the fungus which appeared upon anacharis (G. F. Chantrell).

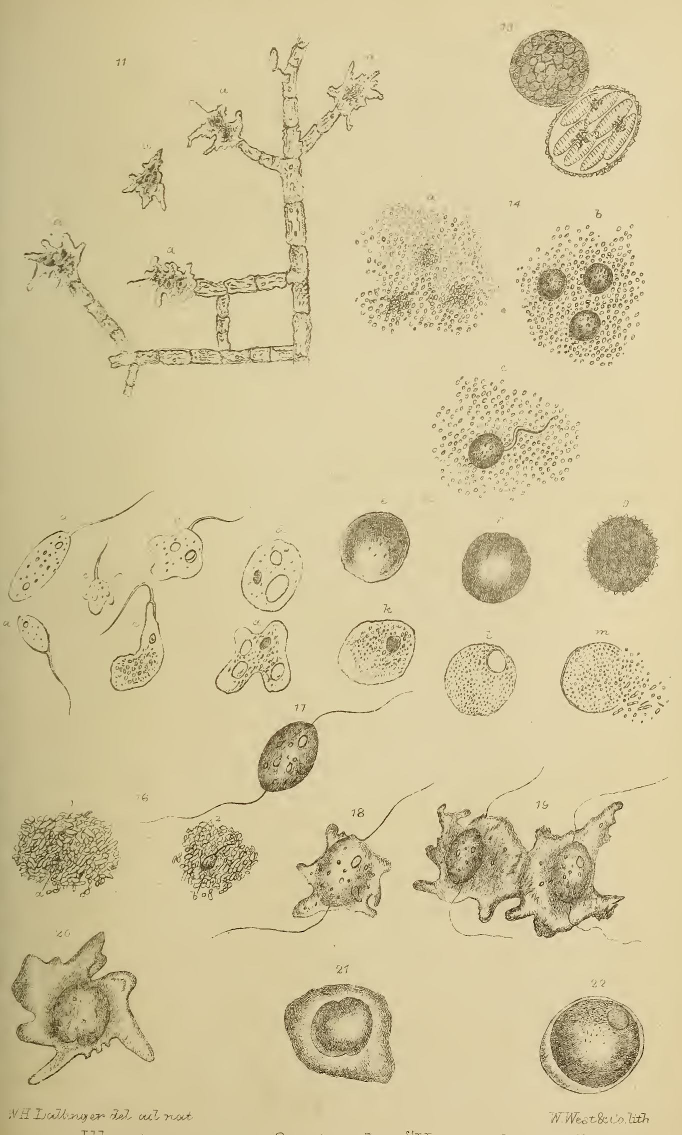
Fig. 12. Supposed case of "heterogenesis" of diatoms from *Cladophora* and algoid corpuscles, *e e′*, *l l′*, *m m′*, but shown by Professor Smith to have arisen naturally in this position, which is their habit.

Fig. 13. A supposed case of "heterogenetic" production of Naviculæ from Euglena, but shown by Professor Smith to be a cluster of diatoms that had been devoured and ejected by an amœba.

Fig. 14. Supposed origin of *Monas lens* in transformed bacteria.

Fig. 15. The reputed "heterogenetic" change of monads into amœba and back again to bacteria (Bastian).

Figs. 16 to 22. The changes in fig. 15, supposed to be heterogenetic, explained by the ascertained metamorphoses of the monad here depicted.



W. H. Dallinger del. ad nat.

W. West & Co. lith.

Illustrations of mistaken "Heterogenesis"

ASTRONOMY IN AMERICA.

By RICHARD A. PROCTOR.

DURING my visits to America in 1873–74 and 1875–76, I was led from time to time to notice with interest the progress and promise of astronomical science in America. My own special purpose in visiting America on these occasions partly brought these matters to my attention. The circumstance that in a country so much more thinly peopled than Great Britain, it should be possible not only to obtain audiences for lectures on such a subject as astronomy, but to obtain more and better and larger audiences by far than could be obtained during a lecture season in England, for any single scientific subject whatever, appeared to me in itself sufficiently remarkable. At a first view this might have been referred simply to the fact that the Americans are a lecture-loving people, preferring the quick and ready method of learning the more striking facts of a subject from a verbal exposition, to close study and application. But I soon perceived that something more than the mere desire for superficial knowledge was in question. The number of persons making close inquiry into the subject was nearly always greater (even in proportion to the much greater audiences), than in England. That still more select section of every audience, the actual workers and observers, I also found to be correspondingly large; while again and again I met with what in England is certainly very unfrequent—cases, namely, in which persons not engaged professionally in the study or teaching of astronomy had privately worked so zealously and so ingeniously in astronomical research as to have effected original discoveries of considerable interest.

I do not propose, however, to enter here into an account of these experiences of my own. To do so would indeed be a welcome task to me, as enabling me in some degree to express not only my sense of the interest taken by Americans in science, but also my recognition of the unvarying kindness with which I was personally received. At Boston, New York, Philadelphia, Washington, Brooklyn, St. Louis, Cincinnati, Baltimore, Chicago, Columbus, Louisville, Kansas, and Minneapolis, and, in fact, at all the cities and towns which I visited, I received a generous and kindly welcome from the community, accompanied by acts of personal kindness from individuals, which I shall always hold in grateful remembrance. But this would not be the place to attempt the task—in any case no easy one—of attempting to express my sense of American kindness and hospitality. My present purpose is to indicate simply the remarkable progress made by Americans in astronomical science during the last half-century.

Fifty years ago there were few telescopes and no observatories in America. It was not greatly to be wondered at that the nation should not up to that time have given any great degree of attention to scientific matters. The proportion of the population having leisure for scientific and especially for astronomical research was but small, and the government had matters of more vital importance to attend to than the erection of observatories. For several years the attention of Congress had been called to the necessity of a national observatory; but when President Adams, in 1825, made a special appeal to this effect, his proposal met with ridicule and disfavour.

The first action towards the initiation of astronomical research in America bears date March 1810, when it was proposed in Congress (by Mr. William Lambert, of Virginia), that a first meridian should be established for the United States (the meridian of the Capitol at Washington being selected), in order to obviate the "confusion already existing in consequence of the assumption of different places within the United States as first meridians, on the published maps and charts" in the country. The proposition was not at once acted upon. In July 1812 we find Mr. Monroe, then Home Secretary of State, indicating its astronomical bearing. "In admitting," said he, "the propriety of establishing a first meridian within the United States, it follows that it ought to be done with the greatest mathematical precision. It is known that the best mode yet discovered for establishing the meridian of a place is by observations of the heavenly bodies; and that, to produce the greatest accuracy in the result, such observations should be often repeated, at suitable opportunities, through a series of years, by means of the best instruments. For this purpose an observatory would be of essential utility. It is only in such an institution, to be founded by the public, that all the necessary implements are likely to be collected together; that systematic observations can be made for any great length of time, and that the public can be made secure of the results of the labours of scientific men. In favour of such an institution it is sufficient to remark that every nation which has established a first meridian has also established an observatory." Mr. Lambert brought in a bill proposing the erection of such an observatory in 1813; but nothing more was done until 1815, when the memorial on which the bill of 1813 had been based was referred to a select committee. No steps were then taken, however, to carry a bill. In November, 1818, a third memorial from Mr. Lambert was presented, and referred to a select committee; but the resolution asked for was not finally passed until March 3, 1821, when Mr. Lambert was appointed by the President, "to make astronomical observations by lunar occultations of fixed stars, solar eclipses, or any approved method adapted to ascertain the longitude of the Capitol from Greenwich." In December 1823 Mr. Lambert, in a report of his labours, gave for the longitude of the Capitol 76° 55′ 30″ ·54, closing his report with a strong appeal for the erection of an observatory.

Two years later, President Adams urged on Congress the establishment of a national observatory as part of a wider scheme for the advancement of knowledge. His remarks on the astronomical portion of his scheme serve well to show the position of astronomy in America half a century ago. "Connected with the establishment of a university," he says, "or separate from it, might be undertaken the erection of an astronomical observatory, with provision for the support of an astronomer to be in constant attendance on the phenomena of the heavens, and for the periodical publication of his observations. It is with no feeling of pride as an American that the remark may be made, that, on the comparatively small territorial surface of Europe there are existing more than one hundred and thirty of these lighthouses of the skies; while throughout the whole American hemisphere there is not one. If we reflect for a moment upon the discoveries which in the last four centuries have been made in the physical constitution of the universe by means of these buildings, and of observers stationed in them, shall we doubt of their usefulness to every nation? And while scarcely a year passes over our heads without bringing some new astronomical discovery to light, which we must fain receive at secondhand from Europe, are we not cutting ourselves off from the means of returning light for light, while we have neither observatory nor observer upon our half of the globe" (!) "and the earth revolves in perpetual darkness to our unsearching eyes?"

In March 1826 a bill "to establish an observatory in the district of Columbia" was brought before Congress and read the first and second time, but the House Journals show no further trace of it. This bill was due to the recommendations of Mr. Adams, who did not relax in his efforts to secure the erection of a national observatory, though delays and disappointments occurred which might well have exhausted his energy, seeing that the dates of his renewed and for awhile useless appeals were 1836, 1838, 1840, and 1842.

Passing over many circumstances in the history of these transactions, not as being without interest, but because space will not permit of their being presented here, we may proceed to the time when the actual erection of the buildings was commenced. This was in 1843, or no less than thirty-three years after the plan for an observatory was first proposed, so that fully one half of the period which has elapsed since Lambert of Virginia first called his countrymen's attention to the necessity of establishing a national observatory was lost in discussions and delays. At the close of September 1844 the new building was ready for occupancy, and the instruments were adjusted.

From 1844 to 1861 the Washington Observatory was under the superintendence of Lieutenant Maury. In September 1846 the first volume of Observations was issued. Its value has been thus described by an impartial and competent judge. "Besides a fair amount of observations with the two transit instruments in the meridian and the prime vertical, and those with the mural circle, it contained various important investigations of the errors and corrections peculiar to the instruments: Professor Coffin's masterly discussion of the adjustments of the mural circle, and his expansion of Bessel's Refraction Tables; Walker's investigation of the latitude of the observatory, and his comparison of the standard thermometers; all of great value."

In the second volume reference was made to the discovery of Neptune, and the success of Mr. Walker, one of the assistants, in detecting amongst Lalande's observations two of Neptune on May 8 and 10, 1795, when the planet was observed and recorded as a fixed star. "Astronomers were thus furnished with an observation of Neptune made 52 years before, which afforded the means of a most accurate determination of the orbit, and enabled the superintendent of the American Nautical Almanac to publish an ephemeris of the new planet two years in advance of all other parts of the Almanac. *The observatory was first brought into prominence by these researches.*" In October 1849 Lieutenant (now Rear-Admiral) Davis wrote as follows to the Hon. Secretary of the Navy on this subject. "The theory of Neptune belongs, by right of precedence, to American science. In connection with its neighbour, Uranus, it constitutes an open field of astronomical research, into which the astronomers and mathematicians of the United States have been the first to enter, and to occupy distinguished places." Deprecating heartily, though I do, all reference to priority or nationality in such matters, as opposed to the true scientific spirit, I cannot but note how Professor Newcomb, by his admirable researches into the theory of Uranus and Neptune, has fulfilled the hopes thus expressed nearly a quarter of a century before his labours were brought to a successful termination.

The work of the observatory thus happily inaugurated was prosecuted steadily till 1861, when Commander Maury left Washington to join the cause of the Confederate States. During the greater part of the war the observatory was under the charge of Captain Gilliss, who died on February 9, 1865. "It has been noted as a strange coincidence of circumstances," says Professor Nourse, in the memoir of the observatory from which we have been quoting, "that the last morning of his life witnessed an announcement of results deduced at this observatory which had fulfilled his long deferred hope of determining the solar parallax by simultaneous observations in Chili and in the United States. This announcement would have been peculiarly gratifying to him because these results were from the joint activity of the two observatories, founded through his exertions, five thousand miles apart."

From 1865 to 1867 the observatory was under the superintendency of Rear-Admiral C. H. Davis, and from 1867 to the present time it has been under that of Rear-Admiral B. F. Sands. Without further considering the work accomplished at the observatory itself, which has partaken of the general character of official astronomical research, we may consider here some of the special astronomical occasions at which the observers trained at Washington have assisted.

The total eclipse of August 7, 1869, was closely observed by parties from the observatory. Professor Asaph Hall and Mr. J. A. Rogers, proceeded to Alaska; Professors Newcomb, Harkness, and Eastman, to Iowa; and Mr. F. W. Bardwell, to Tennessee. The observations made on this occasion were of great value and interest. The solar prominences had had their real nature determined during the eclipse of August 1868; and the American observers were not content to repeat the observations then made, but extended the method of spectroscopic analysis to the corona. They also obtained photographs of the coloured prominences. The work accomplished by the Washington observers, together with the observations made by Dr. Curtis, Mr. J. Homer Lane, of Washington City (Ia.) and Mr. W. S. Gilman, jun., of New York, and Gen. Myer, U.S.A., form a quarto volume of 217 pages, with twelve illustrations. Of this valuable and interesting volume three thousand five hundred copies were printed by joint resolution of Congress.

The superintendent of the Washington Observatory was not content with this. "Believing that the experience of its officers in their observations of the eclipse of 1869 should be availed of for the further elucidation of the subjects involved in such phenomena, he addressed the Navy Department upon the subject of their employment in Europe in observing the eclipse of December 1870; the Department promptly detailed the professors who had been the observers of the previous year;" and it was doubtless through the energy thus displayed by Rear-Admiral Sands, that other skilful American astronomers were enabled to cross the Atlantic for the purpose of observing that important eclipse. Unfavourable weather prevented observations of this eclipse at some of the best stations, but the American observers succeeded in establishing the accuracy of the observations made in 1869; and to them must be attributed in large part the definite demonstration of the fact, which though now admitted was then much disputed, that the corona is a solar phenomenon, and not due to the illumination of our own atmosphere only.

The part taken by the Washington Observatory in preparing for and co-operating in the observation of the transit of Venus, on Dec. 8, 1874, is too recent to need full description in this place. I may be permitted, however, to dwell with special commendation on the manner in which American astronomers devoted themselves at that time to a task which they might fairly have thought the business of their European brethren. A transit of Venus is to occur in 1882 which will be specially American, being visible wholly or in part from every portion of the United States; and if America had reserved her energies for that occasion, no complaint could reasonably have been made. It was indeed the prevalent idea in Europe that that would be the course she would adopt. But with singular generosity and scientific zeal, she not only devoted to the work of observing the earlier transit a sum largely exceeding the amount granted by any other government (and nearly twice as large as Great Britain paid), but undertook some of the most difficult portions of the work, which otherwise would have been left unprovided for. I cannot but recall with a feeling of something like personal satisfaction (though conscious that such a feeling ought to find no place in the mind of the true student of science) the gratification with which I welcomed the announcement, early in 1873, that America had undertaken to occupy positions, the importance of which I had long pointed out, but which, but a fortnight before that announcement reached Europe, had been confidently described as astronomically inferior and geographically unsuitable. The pleasure I then felt was only surpassed by that which I experienced subsequently, when news received from the various observing stations showed that at those just mentioned were achieved some of the most important successes of the occasion.

Another noble contribution made to science at Washington has been the erection of the splendid refractor, 26 inches in aperture, which is now the chief equatorial of the observatory. America is fortunate in possessing in Alvan Clark the greatest living master of the art of constructing large object-glasses of good definition. He had already constructed a telescope 18 inches in aperture for the observatory at Chicago; but by the contract negotiated with him in August 1870 by Professor Newcomb, he was called on to achieve a far more difficult task in the construction of a telescope of 26 inches clear aperture. He has successfully accomplished this task, and the telescope has already obtained good results under Newcomb's skilful management. The most important of these is an extensive series of observations of the satellites of Uranus and Neptune, made with a view of determining the elements of their orbits and the masses of the planets round which they circle. The observation of the two Uranian satellites, Ariel and Umbriel, discovered by Lassell, and of the Neptunian satellite also discovered by him, must be regarded, on account of the extreme difficulty of observing these bodies, as a very valuable contribution to astronomy. It is pleasant to notice that Newcomb has been able most thoroughly to confirm the accuracy of Lassell's work in Malta, the mean motions of Ariel and Umbriel deduced from the Malta observations being so accurate that, says Newcomb, "they will probably suffice for the identification of those objects during several centuries." Although no systematic search has been made for new satellites of Uranus, yet enough has been done to show, "with considerable certainty," that at least the outer satellites supposed to have been seen by Sir W. Herschel "can have had no real existence" (as satellites, that is to say).

Before passing to the brief consideration of the work accomplished in some of the other American observatories, we must fully admit the justice of the remarks made by Professor Nourse in closing his memoir relating to it. "The position now accorded to it," he says, "by the free tributes of scientific men in the Old World as well as at home, is not without honour to our country; and this notwithstanding the comparatively recent founding of the institution, and the as yet limited appropriations sustaining it. It may, therefore, justly claim a yet more generous support; and the pledge may be safely made that if thus supported and efficiently directed, it will make returns yet more gratifying to national pride, and (which is a matter infinitely more important) advancing the highest aims of scientific research. What shall be its future records of success must remain with the support extended by the government and the fidelity of those who are entrusted with its administration."

The actual commencement of astronomical observation in America belongs to a much earlier period than that at which the Washington Observatory was erected. The first telescope used for astronomical purposes in America was set up at Yale College forty-six years ago. The first observatory, however, properly so-called, was erected at Williams College, Mass., in 1836. The next was the Hudson Observatory, established in connection with the Western Reserve College, Ohio, under the charge of Professor Loomis (now of Yale), whose works on astronomy are deservedly held in high esteem in this country as well as in America. Next in order of time came the Observatory of the High School at Philadelphia, which achieved distinction under the able management of Messrs. Walker and Kendall. The West Point Observatory was next established, and placed under the care of Professor Bartlett. All these preceded the Washington Observatory.

Soon after the Washington Observatory had been erected, an observatory was built at Cincinnati. Its history illustrates well the way of carrying out such work in America, when the government does not take the work in hand. The idea of erecting an important observatory in Cincinnati was first entertained by Professor Mitchel, then professor of mathematics at Cincinnati College. He proposed to attempt the task without any aid from the general or state government, by the voluntary contribution of all classes of citizens. To ascertain whether any interest could be excited in the public mind in favour of astronomy, he delivered in the spring of 1842 a series of lectures in the hall of Cincinnati College. With truly American ingenuity he devised a mechanical contrivance, by help of which telescopic views in the heavens were presented with a brilliancy comparable with that "displayed by powerful telescopes." These lectures were attended by large audiences, and, I may add in passing, that the interest which they excited is to this day well remembered in Cincinnati—no small proof of Professor Mitchel's power as a lecturer.* The last lecture of the course was delivered in one of the great churches of the city (a thorough American and sensible proceeding), and at the close Professor Mitchel submitted to the audience, consisting of more than two thousand persons, his plan for erecting a first-class observatory, and furnishing it with instruments of the highest order. He promised to devote five years of faithful effort to accomplish this task. The following course was then suggested:—"The entire amount required to erect the buildings and purchase the instruments should be divided into shares of twenty-five dollars; every shareholder to be entitled to the privileges of the ob-

* The same remark applies to the lectures which he subsequently delivered in New York, New Orleans, Boston, Brooklyn, and other large cities. It is almost impossible to over-estimate the service thus rendered by Professor Mitchel to astronomy in the United States.

servatory under the management of a board of control, to be elected by the shareholders. Before any subscription should become binding, the names of 300 subscribers should be first obtained. These 300 should meet, organise and elect a board, who should thenceforward manage the affairs of the association." In three weeks the 300 subscribers had been obtained, without calling any public meeting, and merely by quiet visits in which the nature of the scheme was described and explained. Then officers were elected, a directory formed, and Mitchel was sent "to visit Europe, procure instruments, examine observatories, and obtain the requisite knowledge to erect and conduct the institution, which it was now hoped would be one day reared."

When Mitchel returned four months later, a great change had occurred in the commercial affairs of America. "Everything was depressed to the lowest point," and it was with great difficulty that a sum of 3,000$ was collected and remitted to meet the first payment for the telescope of 12 inches aperture ordered of Merz. The best place for the observatory was a hill-top rising 400 feet above the level of the city. On offering to purchase this from Mr. Longworth, to whom it belonged, Professor Mitchel was directed to select and enclose four acres, which Mr. Longworth presented to the association. On Nov. 9, 1843, the corner-stone of the pier which was to sustain the great refracting telescope was laid by John Quincy Adams, who undertook the long (and then difficult) journey from Washington to give this proof of his interest in the cause of astronomy. When, in May 1844, the great telescope was paid for, the funds of the association were exhausted, and the estimated cost of the building amounted to more than 7,000$. In this difficulty a simple but again perfectly American plan was followed. Mechanics and others were invited to subscribe for stock in the Astronomical Society, paying their subscriptions with work. In six weeks not less than one hundred hands were at work on the hill-top and in the city. The stone of which the building was erected was quarried from the grounds of the society. The lime was burned on the hill, and every means was adopted to reduce unnecessary expenditure. Payment for stock was received in every possible article of trade; due bills were taken, and these were converted into others which would serve in the payment of bills. In this way the building was reared, and finally covered in, without incurring any debt. But the conditions of the bond by which the lot of ground was held required the completion of the observatory in June 1845. It was seen to be impossible to carry forward the building fast enough to secure its completion by the required time without incurring some debt. "My own private resources," proceeds Mitchel, "were used in the hope that a short time after the finishing of the observatory would be sufficient to furnish the funds to meet all engagements. The work was pushed rapidly forward. In February 1845 the great telescope safely reached the city; and in March the building was ready for its reception." Unfortunately, just at this time, when his private means were exhausted, Professor Mitchel's professorship was brought, in a very summary manner, to a temporary close, in consequence of the college edifice being burned to the ground. To recruit his means without abandoning the cause of astronomy, he gave courses of lectures in the chief cities of the United States, meeting with well-deserved success.

The observatory thus erected achieved useful, though not very striking results. An observatory which was erected a year or two later took so quickly the leading position, so far as the actual study of the heavenly bodies was concerned, that the progress of the Cincinnati astronomers, as indeed of most of the astronomers of the United States, received less attention than otherwise might have been the case. I refer to the Observatory at Harvard (Cambridge, Mass.). Here one of the first equatorials ever made by Merz was erected; and by means of it W. C. Bond and his son Geo. P. Bond made highly interesting additions to astronomical knowledge. The seventh satellite of Saturn (eighth and last in order of discovery) was detected, the dark ring rediscovered and found to be transparent; important drawings of nebulæ were made, and many other observations were effected, under the administration of the Bonds. Later, under Professor Winlock, the Harvard Observatory has been distinguished by the excellence of the mechanical arrangements adopted there, and by M. Trouvelot's admirable drawings of solar spots and prominences of the planets Jupiter and Saturn, and of various details of lunar scenery.

In passing, I may note that at Harvard, as indeed elsewhere in America, others than professed astronomers have achieved very useful astronomical work. As Professor Mayer, of the Stevens Institute, Hoboken, has turned his marvellous ingenuity in devising new methods of physical research to astronomical inquiries, so Professor Cooke of Harvard, whose special subject is chemistry, made a most important astronomical discovery, which has since been ascribed to Janssen, who, later (though independently and by another method) effected it. Professor Cooke made a series of observations on those bands in the solar spectrum which are due to our own atmosphere, with the object of ascertaining whether they are due to the constant constituents of the air, or to the aqueous vapour which is present in the air in variable quantity. Combining hygrometric with spectroscopic observations, he found that when the air is moist these bands are more clearly seen than when the air is dry, and by systematic observations so definitely ascertained this relation as to prove beyond all manner of doubt that the bands are due to aqueous vapour. Unfortunately, though his results were published in America, they were not published in such a way as to attract notice in Europe, and accordingly European astronomers remained ignorant of the most important fact discovered by Cooke until they had rediscovered it for themselves.

The Observatory at Ann Arbor, Michigan, was erected in 1854, chiefly through the exertions of Chancellor Tappan, of the Michigan University. Dr. Brünnow, our present Astronomer Royal for Ireland, was for a long time director of this observatory. It is at present under the able control of Professor Watson, who has added nearly a score of planetoids to the known members of the solar family.

The Observatory of Dartmouth College, Hanover, N.H., illustrates in a remarkable way the energy and zeal with which college observatories are managed in America. It would be difficult to name any observatory in this country where observations of greater interest, as respects the physics of astronomy, have been made than those effected by Professor Young with the 9-inch telescope constructed by Alvan Clark for the Dartmouth College; or than the supplementary observations made by Young with a powerful telescope conveyed to an elevated pass in the Rocky Mountains. Amongst his results may be specially mentioned—first, the observations of the most remarkable solar outburst yet witnessed, an outburst during which the glowing hydrogen of the prominences was driven to a height of at least 200,000 miles from the surface of the sun; and, secondly, the identification of more than 250 lines in the spectrum of the solar sierra.

And as the most interesting and characteristic observations yet made upon solar prominences are due to Professor Young of Dartmouth Observatory, so the most accurate and detailed drawings yet made of sun-spots are those by Professor S. Langley, of the Alleghany Observatory, near Pittsburgh.

At Chicago, a very fine telescope, 18 inches in aperture, by Alvan Clark, has been erected; but, owing to pecuniary difficulties consequent on the great fire (followed by the commercial depression which has recently affected the United States), that observatory has suffered considerably from the want of a properly remunerated director. The Astronomical Society of Chicago has done its best to set matters straight, but differences have arisen which have marred their efforts. In the meantime Mr. S. W. Burnham, of Chicago, has shown admirable zeal and skill in the systematic observation of double stars, having discovered and measured more than 450 of these objects (all of a delicate and difficult nature).

But, indeed, it would be hopeless to attempt, in the short space available to me here, to give any sufficient account of the labours of American astronomers, whether attached to government or state observatories, or working independently. Of the latter, and in my opinion not the least important class, I need cite only Drs. Rutherford and H. Draper, the former of whom. besides making other extremely important contributions to astronomy and physics, has produced celestial photographs admittedly better than any obtained on this side of the Atlantic, while the latter at an earlier period achieved results in celestial photography which were far superior to any obtained at that time, or for many subsequent years. The advice and assistance rendered by Dr. H. Draper to the astronomers to whom was entrusted the preparations for the recent transit, was most deservedly commemorated in a medal which the American government honoured itself by awarding to him.

The most striking feature in the contributions made by Americans to astronomy appears to me to be the skill shown in noting the essential points to be aimed at, and the fertility and readiness of resource exhibited as the work proceeds. In England, students of astronomy are too much in the habit of following conventional rules and wasting time over unnecessary preliminaries. An American astronomer notes that some particular observation is wanted, and directs his efforts to making that observation, not considering it necessary in the first place to go over ground already repeatedly traversed by others.

I have been sometimes asked whether officialism is as rampant in America as in England in matters scientific. American scientific officials have assured me that it is, or rather (for they have not worded the matter precisely in that way) they hold that official science is properly (as they consider) paramount in their country. I was gravely assured in Washington, for instance, that the course which I had pursued in England with reference to the suggested official schemes for observing the transit of Venus in 1874 would never have been tolerated in America, despite the fact that the course actually followed by American official science was precisely that which I had advised. It was the *principle*, so an eminent American official scientist assured me, which was in question, and no American would have been suffered to oppose as I did the course advised by the chief official astronomer. What would have happened to such an unfortunate was not clearly indicated; and I must confess that all I heard outside official scientific circles in America suggested to me that any mistake made by official science would be commented upon even more freely in America than in England, and quite as safely. In fact, I had reason to believe that the warmth of my own welcome in America was in no small degree due to the fact that having first proved the justice of my views, I had not been afraid to maintain them publicly against the powers that were until the proper course was adopted.

One other point remains to be noticed—the influence, namely, of religious scruples upon scientific progress and research in America. Here I must admit that I was somewhat disappointed. I expected to find America a long way in advance of England. But with some noteworthy exceptions, especially in the west, America seems to me to be behind England in this respect. It is only here and there in England—in the Bœotian corners, so to speak, of this country—that the community opposes itself to advanced scientific ideas to the same extent as in some of the leading cities of the United States. This is partly due to two opposite influences—the Puritan element of the American population on the one hand, and the Roman Catholic element on the other. Progress, however, is being steadily made in this as in other matters. Indeed, it has been rather because America began later to bestir itself in the encouragement of free search after truth, that she is at present behind England in this respect. Judging from experience in other matters, she will move rapidly now her progress has begun, and will soon occupy the position to be expected from the natural freedom and independence of the American mind. It need hardly be said, that in America as in Europe, such contest as arises from time to time between religion and science has its origin entirely from the side of religion. There, as here, religion (so-called) attacks and denounces discoveries inconsistent with the views which the orthodox had been accustomed to advocate; and there, as here, when there is no longer any choice, the orthodox quietly accept these discoveries as established facts, expressing a *naïve* astonishment that they should ever have been thought in the least degree inconsistent with received opinions.

ON THE PROGRESS OF AERONAUTICS.

By FRED. W. BREAREY,

HON. SEC. TO THE AERONAUTICAL SOCIETY OF GREAT BRITAIN.

TO the casual observer of a balloon which floats away from his presence into the dim distance, amidst the cheers of the crowd, and from thence into the solitude of an infinite space, it is hard to believe that its utility is doomed to the limit of mere flotation. He thinks that there is either an immense amount of apathy or else a lamentable display of ignorance among mechanical minds which prevents their energies being concentrated upon the navigation of the balloon. This is of course the popular judgment, yet it is only partially erroneous. The late Franco-German war afforded an opportunity for energy and engineering capabilities, and we know something of what balloons are capable, though perhaps not the uttermost, especially when accompanied by unlimited expenditure. The termination of the war interfered with certain designs, for the accomplishment of which M. de Fonvielle had escaped from Paris in a balloon. As he was the chief of the Aeronautical Department, he hoped to collect at Lille all the balloons which had left the French metropolis, and he himself came to England with the object of consulting as to the best means of aiding the return journey.

When the armistice was concluded six balloons had been collected at Lille, waiting for a favourable wind. By the aid of a small propelling force M. de Fonvielle believed that, starting with a fair wind, he would be able to deviate a few degrees from the current if necessary. It is highly probable that the return would have been effected, as Paris was the centre of a circle of investment of twenty miles diameter. With a favourable wind a sailor named Gally, with three others, left Paris on Nov. 12, with the intention of reaching Bordeaux, and they descended at Gondreville, *near* Bordeaux.

There is an anomalous incidence connected with the subject which would seem to favour the advocates of balloon propulsion, viz., that although water is about 800 times denser than air, yet the rarer atmosphere is capable of supporting the heavy bird, whilst the fish is about the same density as the element which it inhabits. They forget, however, that the currents to which the denser elements are liable, and with which the fish can cope with ease, are limited to eight, or at most to ten miles an hour, but where might the balloon be in a similar period? Possibly eighty miles away. Let us suppose a boat under the same conditions as often appertains to a balloon, viz. in a current of twenty miles an hour. The river, let us say, is five miles across, and we can propel our boat at the rate of five miles an hour, as M. Dupuy de Lome has lately accomplished in France with a balloon. The boat is accordingly propelled to the other side in one hour, but in that time it has drifted twenty miles down stream.

Now by powerful machinery it is just possible to propel a rigid construction like a boat against such a current with perfect impunity, but to attempt the same thing with a balloon is an abandonment of all scientific principles. Any attempt to force the balloon unnaturally into the condition of the boat must be made at the risk of a burst-up.

In the case of the boat, the thrust of the propeller is in the same line with the centre of gravity and displacement. In the case of a balloon the propeller works from a pendant car considerably below the centre of displacement.

If it were possible, therefore, to propel the balloon at a rate which bore a more suitable proportion to the force of the currents to which it is liable—let us say at the least twenty-five miles an hour—we may, without travelling with it to note the effect, just imagine the balloon anchored to the ground from the car, and a wind of twenty-five miles an hour blowing against it, which is precisely the same thing. The balloon would be forced into an inclination of about 60° out of the perpendicular.

The dimensions of a balloon intended to carry a propeller with the men or machinery to work it, could scarcely be less than 60 feet in height by 50 in breadth, and taking the mean it would be equal to a sphere whose largest section would contain about 2,372 square feet. If we take two-thirds of this, viz. 1,581 square feet—because remember that it is a compressible sphere—we shall obtain a surface upon which the whole force of the wind at twenty-five miles an hour will be expended; and at that rate, by Rouse and Smeaton's tables, the pressure upon each square foot exposed would be 3·075 lbs., equal to a total force of 4,861 lbs., or upwards of two tons. We find, therefore, the pressure to which a balloon of that capacity would be exposed were a motive power furnished capable of propelling such a balloon at twenty-five miles an hour; and we learn also the angle of inclination which the car would assume relative to the balloon, the car in this case being in advance. Of course a more suitable shape of the balloon would modify these conditions, but only to produce other elements of difficulty which cannot be overcome. For instance, as we depart from the spherical shape of the gas-holder the buoyancy decreases and its weight increases! And particularly if some elongated form be constructed with conical or cigar-shaped ends, then struts must be employed to preserve its shape, and the extra weight to be thus sustained requires a balloon of such gigantic dimensions that, merely to inflate it, involves a cost quite out of proportion to any attainable result. No conditions except the exigencies of warfare would warrant the necessary expenditure. If any such conditions should ever attach to us as a nation, doubtless English talent will be equal to the emergency. Also if it be found desirable to escape from any besieged place, there will always be found sufficient of English body-linen, both male and female, for the whole population to float away, though perhaps in a makeshift manner.

The apparent anomaly before alluded to, of the lighter element and the heavy bird—the denser element and, by comparison, the light fish—is capable, however, of explanation, and its consideration will assist us in determining the conclusions at which the Aeronautical Society has arrived, viz. that flight is purely a mechanical action capable of imitation; that it is unassisted by air-cells or other contrivances for effecting levity, and that the balloon is incapable of being rendered useful to man as a means of locomotion, except in the way of waftage, and that this mode of progression in relation to the earth is capable of being materially assisted by some method of raising or lowering the balloon at pleasure, without loss of gas or ballast.

Let us suppose that the fish bears the same proportion of weight to its elemental medium as does the bird to the air. Judging from the fact that birds have been observed when shot to sink half their bulk in water, they may be taken to be about half the weight of water, and therefore they are about 400 times heavier than the bulk of air which they displace. If, then, the fish were equal in weight to the heaviest material of which we know, viz. platinum, it would still be light in comparison to the bird in the air. For instance, a cubic foot of platinum weighs 20,000 ozs; one of water weighs but 1,000 ozs.

We have estimated the bird as being half the weight of water, equal to 500 ozs. the cubic foot, whilst one of air is only about 1·285 ozs. Therefore the instant fall of the fish to the bottom could only be prevented by such appendages as wings, and the facility to manipulate them, which, however, the density of the element in which they exist entirely precludes.

The fish, then, would fall to the bottom like a lump of platinum without the ability to rise. It is the bird without wings, for the same thing would happen to the bird, did it not possess the ability to convert the force of gravitation into horizontal force by the manipulation of its wing surface, for it thereby covers and controls the weight of air, which in a given time is included, and passed over, within the breadth of the wing-tips. If necessary, gravitation may be still further diverted by the impact of the wing upon the air, because the resistance of the air bears a certain accelerated proportion to the rapidity with which it is displaced. This fact, so important to the hopeful student of aeronautics, is simply illustrated by the alternately slow and rapid waving of any light plane surface, such as a fan.

We have seen, then, that the fact of the specific gravity of the fish, and that of the element in which it disports itself, being nearly the same, presents no encouragement to the employment of the balloon as a means of locomotion, but it does afford encouragement for the adoption of the only suggestion made in its favour by the Aeronautical Society, because the fish possesses the power, within a narrow limit, of making itself lighter or heavier, and this is just the quality which we desire for the balloon, but to which, independently of throwing out ballast, or parting with gas which cannot be replenished, we have not yet attained.

In this direction, therefore, lies the one improvement of which the balloon is capable, viz. the means to secure its ascent and descent without expenditure of gas or ballast.

The power required to raise a mass which already possesses buoyancy is very slight compared with that which is requisite to propel against a resistant atmosphere. The effect also of the more simple power would be incomparably greater, because upon the supposition that a balloon required an additional power of 20lbs. beyond the gas with which it is inflated to raise it into the air, the application of a slight mechanical arrangement would clear it from all obstructions and bring it under the influence of another power, viz. the air in motion which would give it horizontal direction, and if that direction is not the one desired, it might raise it in search of another. The cessation of the mechanical action would also bring it down to the ground. Apply this power equal to 20 lbs. to a propeller working horizontally, and its inadequacy to effect any satisfactory result in a direct line becomes apparent. In comparison with the space to be travelled, the rate at which propulsion could be attained would be utterly insignificant.

Here let the imagination have a little play. For purposes of enjoyment it is not necessary to mount into the clouds. Our aerial yacht requires but to skim the trees and buildings. Wafted across the wide expanse, it may skim the fields and rise to the hedges. With an envelope more impervious to the escape of gas, the voyage may be prolonged. Floating day after day, at the caprice of the wind certainly, but always over fresh scenery, nothing could be more enjoyable.

During these excursions it would be possible to renew the gas by proximity to some gas works, and not seldom it might happen that, by waiting for it at anchor, a favourable wind might influence a return journey. It may here be remarked that any mechanical means such as a screw acting vertically in the car of a balloon, capable of exerting a force of thirty-five pounds, would save one thousand cubic feet of gas.

It was intended by the society which I represent to try the effect of the screw acting vertically upon a balloon, but in the meantime the War Office authorities were induced to give a trial to the inventor of a screw by which he hoped effectually to propel the balloon. He was induced to attach also an arrangement which acted vertically. The following is an extract from Captain C. Orde Browne's article, published in the "Popular Science Review" for October 1874.

"The difficulty of ascertaining exactly when a captive balloon is balanced, when even a slight wind is blowing, so as to stretch the retaining rope, made the first trial a little doubtful; and after one ascent, apparently due to the working of the propeller, a doubt arose as to the exact balance of the balloon, which might have a tendency to rise, and only have been held down by the captive line, which, except at very still moments, was pulled taut by the wind acting on the balloon. It being ascertained at a still interval that the balance was good, the vertical gear was worked, and the balloon again rose. The rate of ascent was difficult to estimate; it was judged, however, not to exceed fifty feet a minute. A positive indication of the power of the propeller was thus obtained, and it should be noticed that the circumstances—if the rate of ascent only was measured—were rather disadvantageous, for the weight of the line up to the extent of forty feet was gradually added to the balloon as it rose. Had the mean rate of ascent and descent been taken, this error would be eliminated, for the descent would be favoured by the weight of the rope from forty feet in length at the maximum height, down to nothing at the ground."

The balloon was now liberated, and the report goes on to say: "The horizontal gear, however, throughout the entire voyage failed to give any satisfactory results; even allowing that the effect was perceptible, it is impossible to lay much stress upon it. Any force would give a perceptible effect if recorded with sufficient delicacy. There is no use in an insignificant effect unless it can be shown that means exist by which it could be increased sufficiently to bear a reasonable relation to the forces to which it is to be opposed."

Therefore the experiment which the society had advocated for several years, and which it had at last determined to adopt, having proved successful under disadvantageous circumstances when tried by others, the apparatus ordered was countermanded. The opinion which had been expressed also in the various reports issued by the society as to the inutility of any screw apparatus for effecting horizontal propulsion was confirmed.

It is singular that no one has taken advantage of an ascertained fact to put the balloon to more pleasurable, because more prolonged use, than has hitherto been attempted. For instance, let us consider the mode of propulsion adopted in a punt—a clumsy kind of boat, which may often be seen moored in the Thames, in mid-stream, with harmless old gentlemen seated therein, alluring gudgeon. Well, these punts are cleverly managed with a long pole. In a rapid stream, should the punter wish to go with it, he has nothing to do but to keep it off from the bank, under the full influence of the stream; and there is every probability that with a balloon so balanced a push with a long pole would send it up spinning for fifty feet or more, and one might traverse a few hundred yards before it neared the earth and required another push. The distance traversed between each push would of course depend upon the velocity of the air current. It is evident that no ballast is necessary under such conditions, therefore the absence of that would allow of reduced size of balloon. All this, however, is simply waftage.

It is believed by some that the screw may yet serve a more useful purpose than that of the translation of a merely buoyant body. By muscular effort alone all that has been done by the power of one man has been the raising of 26½ lbs. weight.

There has latterly been a more ambitious attempt, involving the expenditure of several hundred pounds of money.

It resulted from the experiments which the Aeronautical Society instituted with a view to record for the benefit of inventors the exact lifting pressure due to the wind advancing against a plane inclined towards it at different angles. These experiments, which took place at Messrs. Penn's factory, at Greenwich, were conducted by several well-known members of the council, and it was well understood at the time that if the results gave no encouragement for the attainment of success in utilizing the air as a highway, the society should be dissolved.

Accordingly an instrument devised by Mr. Wenham was constructed by Mr. Browning for the before-mentioned society, and submitted to a powerful blast from a fan-blower ten feet long by eighteen inches square.

The direct pressure upon one foot square of steel plate, with the blast acting at right angles to the plate, was 3·24 lbs., which, according to Rouse and Smeaton's tables, evidences a wind velocity of about twenty-five miles an hour. The same plate inclined at an angle of 15° from the horizontal, received a direct pressure of only 0·33 lbs., accompanied by a lifting pressure of 1·5 lb. There were various inclinations and different areas tried, but there is no need here to go fully into the tabulated results. It will be sufficient to say that a plane of one square foot, impelled at an angle of 15° against air moving at the rate of about twenty-five miles an hour, will support a weight of one pound and a half, whilst it will only meet with a resistance to its forward motion of five ounces and a quarter, although of course there would have to be added to this the resistance due to the form in which the weight is disposed. A less angle than 15° could not be tried owing to some obstruction in the action of the instrument, but the experiment shows very great lifting force in proportion to the power which requires to be expended in the propulsion of the plane. It shows also that the ratio of the lift to the thrust greatly increases as the inclination diminishes, whilst the force to propel is equally lessened, and thus the sustained flight of birds, often with motionless wings, is in great part accounted for. There exists also another circumstance which is favourable to the extension of the sustaining surface, viz. that the lifting power relative to the square foot increases in some yet unknown ratio with the extent of surface exposed, upon the principle which has been ascertained that the more the total area of a ship's sails is cut up into portions, the less the effect.

Such fundamental experiments accord with the legitimate duty of the society to which I have the honour and pleasure to act as honorary secretary. It is left to the spontaneous efforts of individual members to work up to the data thus established.

Upon such men, whilst the world generally look with amused pity, the eyes of a cautious and watchful few are fixed, ready to take advantage of the first hopeful result. It is my earnest hope that the society will stand between such men and injustice at the time of the general scramble for pecuniary recompense.

It will be the place here to allude to the late and very expensive attempt of Mr. Thomas Moy to construct an apparatus by means of which, in his trials in order to obtain a fulcrum upon the air, he practically tested the correctness of the facts brought out and tabulated.

Mr. Moy was an exhibitor at the Exhibition of the Society at the Crystal Palace in the year 1868. For some purpose or another, ignoring Mr. Stringfellow's light engine, which then gained the prize, he commenced to design another which he deemed more suitable for his object. This was to actuate four driving-wheels, ten inches in diameter, to act in their turn upon two aero-plane wheels, six feet in diameter, by frictional gearing on the periphery. The aero-plane wheels had each twelve light wooden planes fitted to them, something like the screw-propeller, but with the important difference that the pitch was variable at every portion of the revolution. His theory was, that the action of these planes in their revolution through the air was a perfect mechanical imitation of the action of a bird's wing in the various positions that its surface assumes during the progress of flight, giving, as it does, an upward and forward thrust continually, without any downward force from the air on any of the aero-planes. The whole apparatus was placed upon wheels.

The steam-engine was contained in a case 27 inches by $27\frac{1}{2}$ inches by $7\frac{1}{2}$ inches. The diameter of its cylinder was $2\frac{1}{8}$ inches, length of stroke 3 inches. The generating tubes contained a surface of eight square feet, and in these tubes the water circulated with very great rapidity, and thereby utilised the heat in the very best manner. There was literally no framing to the engine itself, as the driving-axle ran in a tube which passed through the steam-chest. The steam was cut off at half-stroke by the slide-valve, and with a pressure of from 120 to 160 lbs. to the square inch, and 536 revolutions per minute, it might fairly be called a 3-horse-power engine. The fuel used was methylated spirits under pressure, which produced blow-pipe flames.

With regard to the form of the so-called "aerial steamer," which was tried in March 1874, we may say that the engine was fixed about four feet from the ground, on a framework which also held the lamps. Other frames, extended from this on each side, take the axles of the 6-feet driving-wheels. These axles were 3 feet 3 inches in length and $1\frac{1}{2}$ inch in diameter, made of drawn brass tube, and very light and strong. An aero-plane of 50 square feet surface was fitted in front, and one of 64 square feet behind.

These were inclined at an angle of 10° from the horizontal; and Mr. Moy calculated that if the whole could be driven on the ground at thirty-five miles an hour, it would encounter a pressure from the atmosphere sufficient to support the whole weight of the machine, which amounted with its engine, fuel, &c., to 214 lbs. The driving-surface of the revolving aero-planes was equal to 60 square feet.

Before running the "aerial steamer" round the central fountain at the Crystal Palace, Mr. Moy took steps to test the correctness of the principle upon which he had been working, guided by the experiments undertaken by the Aeronautical Society. If the old theory was correct, he argued, the lifting pressure on the planes would only amount to a few ounces per square foot; if the new theory was correct, the pressure would far exceed that of the old. It turned out that the old theory was wrong, and the reliability of the recent experiments was confirmed. The revolving planes having been set at an angle of 15°, the pressure was found to be exactly one pound to the square foot at a speed of twenty miles an hour; and with the angle set at 45°, the pressure was $1\frac{1}{2}$ lb. to each square foot. The success of the ulterior experiment, therefore, all depended upon whether he could obtain a sufficient speed upon the ground to avail himself of the lifting-pressure due to the angle of inclination.

The fountain had a path round it. The diameter of the circle was 300 feet. A pole was erected in the centre, from the top of which two cords were attached, one to each end of the machine. Though the gravel had been rolled, the action of the machine under steam was so rough and unsteady that the experiment had to be abandoned until a suitable road could be constructed.

This was eventually effected with 8,000 square feet of boarding, lent by the Crystal Palace authorities, when, after its occupation for some time by the snow, the roadway was ready for a further trial. Instead, however, of the necessary speed being attained, viz. thirty-three miles an hour, it was only possible to get about twelve, so that it was felt as a matter of regret that arrangements had not been made to run it upon a straight line of railway. The wheels, fitted for forward motion only, offered great resistance to running round a circle.

This machine, however, weighing nearly two cwt., was impelled round a circle at twelve miles an hour by the pressure of two aero-plane wheels working in the air, an achievement I believe to be quite unprecedented.

This first start from the ground has always presented great difficulties to the experimenter in aeronautics. Theory has generally favoured the incline as the readiest mode of accomplishing the object, but it is certain that unless there is power sufficient to raise the weight, safety in controlling the descent under exceptional circumstances cannot be secured, as the parachute form would be out of character in any machine designed for rapid transit through the air. In the difficulty in which Mr. Moy found himself, it was natural that he should turn to the vertical screw.

In the report of the Aeronautical Exhibition in 1868, drawn up by Mr. Wenham, the following paragraph appears:—

"Though we are still without a precise demonstration of the power required for flight in the way that a bird flies, the force to maintain which, in some species, must be very small, yet we have some evidence of the power required to lift a weight in the air by means of vertical screws. By this method it has been demonstrated that 100 pounds may be supported by a constant force of about 90,000 foot-pounds, or three horse-power.

"Now, in the work of Mr. Stringfellow, the society has brought out the remarkable fact that a one-horse power engine can be made to weigh only 13 lbs., thus showing the possibility of obtaining flight by the repudiated system of vertical screws, even with the enormous expenditure of power that this plan is known to require."

In order to ascertain what actual lifting power could be obtained with planes moving in horizontal orbits, Mr. Moy constructed new aero-plane wheels, 12 feet in diameter, with twelve planes to each wheel, the whole presenting 160 square feet of surface, driven by a steam-engine weighing 80 lbs. By placing the whole acting surface on these two wheels, an interesting experiment was carried out.

It was palpable, however, that from the conditions of the actual trial the full lifting-power due to the surface, angle, and velocity could not be hoped for. These revolving planes were travelling all the time in one circle. They had not the advantage of obtaining an abutment upon a previously undisturbed body of air. The experiment was in an enclosed part of the building. Great part of its power was expended in drawing downwards a body of air. The whole weight of the machine was 186 lbs. Levers were attached to the spindle of the aeroplane wheels, which were weighted to take off all over 120 lbs. This latter weight was raised from the floor—according to the independent testimony of Captain Greenfield, of the Royal Artillery—as much as six inches under one aero-plane, and two inches under the other, this inequality being due to one wing-plane having broken.

The engine, therefore, was proved capable of raising itself, and 40 lbs. additional weight, under great disadvantages. The revolutions of these two 12-feet aero-planes were sixty-seven per minute.

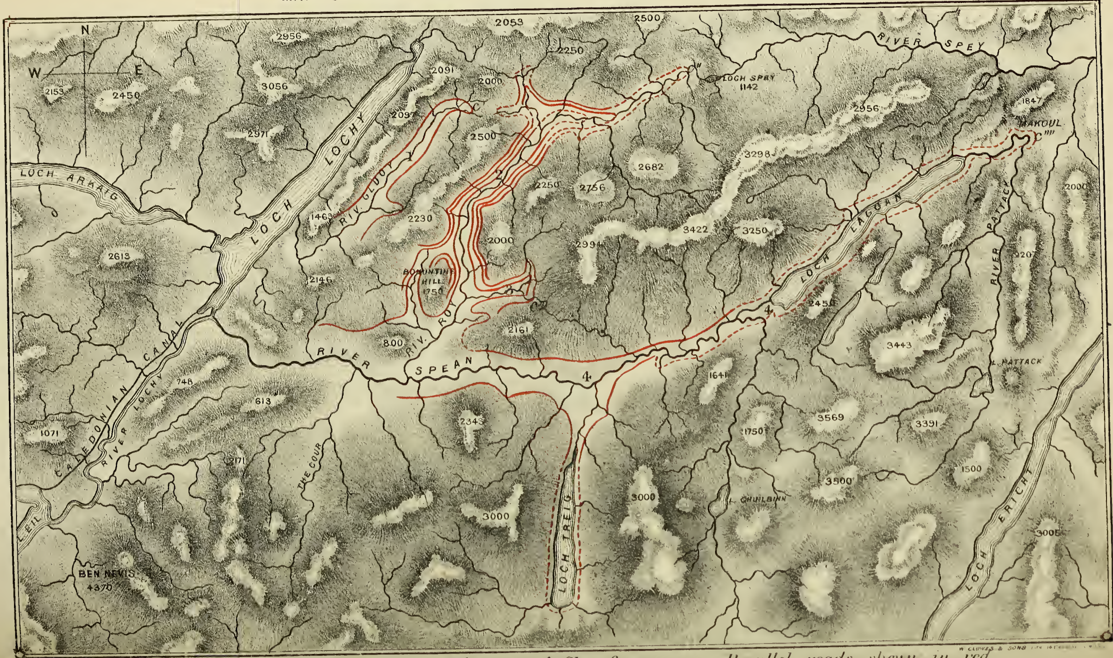
The preparations for the experiments, which have here so easily been summed up, occupied winters and summers. Repeated breakages, renewals, strengthenings, re-construction, re-adjustment, both in engine and apparatus, testify to the patient perseverance of the inventor, and those associated with him. And though lastly mentioned, yet by no means the least, was the constant leakage of the not too auriferous money-bag.

This, however, is the common lot. The situation can only be estimated by the spectator—such as I am—of many abortive attempts.

My space is consumed before my pen has run dry, else I might adduce some more reasons why such earnest workers in the solution of the profoundest problem which ever absorbed the brain-power of aspiring man should be encouraged by the wealthy to go on and progress in Aeronautics.

MAP SHEWING THE PARALLEL ROADS OF GLEN ROY.

Plate CXLI.



1. Glen Gluoy 2. Glen Roy – 3. Glen Glaister – 4. Glen Spean Parallel roads shewn in red

Heights marked in feet 0 1 2 3 4 5 6 7 8 9 10 Scale of Miles.

THE PARALLEL ROADS OF GLEN ROY.

By Professor TYNDALL, D.C.L., LL.D., F.R.S., &c.

[PLATE CXLI.]

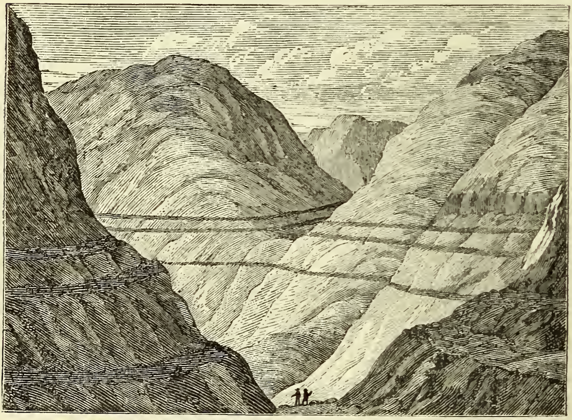
THE first published allusion to the Parallel Roads of Glen Roy occurs in the appendix to the third volume of Pennant's "Tour in Scotland," a work published in 1776. "In the face of these hills," says this writer, "both sides of the glen, there are three roads at small distances from each other and directly opposite on each side. These roads have been measured in the complete parts of them, and found to be 26 paces of a man 5 feet 10 inches high. The two highest are pretty near each other, about 50 yards, and the lowest double that distance from the nearest to it. They are carried along the sides of the glen with the utmost regularity, nearly as exact as drawn with a line of rule and compass."

The correct heights of the three roads of Glen Roy are respectively 1,150, 1,070, and 860 above the sea. Hence a vertical distance of 80 feet separates the two highest, while the lowest road is 210 feet below the middle one.

These "roads" are usually shelves or terraces formed in the yielding drift which here covers the slopes of the mountains. They are all sensibly horizontal, and therefore parallel. Pennant accepted as reasonable the explanation of them given by the country people, who thought "they were designed for the chase, and that the terraces were made after the spots were cleared in lines from wood, in order to tempt the animals into the open paths after they were rouzed in order that they might come within reach of the bowmen who might conceal themselves in the woods above and below."

In these attempts of "the country people" we have an illustration of that impulse to which all scientific knowledge is due—the desire to know the causes of things; and it is a matter of surprise that in the case of the parallel roads, with their weird appearance challenging inquiry, this impulse did not make itself more rapidly and energetically felt. Their remoteness may perhaps account for the fact that until the year 1817 no systematic description of them, and no scientific attempt at an explanation of them, appeared. In that year Dr. MacCulloch, who was then President of the Geological Society, presented to that Society a memoir, in which the roads were discussed, and regarded as the margins of lakes once embosomed in Glen Roy.

To Dr. MacCulloch succeeded a man, possibly not so learned as a geologist, but obviously fitted by nature to grapple with her facts and to put them in their proper setting. I refer to



Parallel Roads of Glen Roy.
After a Sketch by Sir Thomas Dick-Lauder.

Sir Thomas Dick-Lauder, who presented to the Royal Society of Edinburgh, on the 2nd of March, 1818, his paper on the Parallel Roads of Glen Roy. In looking over the literature of this subject, which is now copious, it is interesting to observe the differentiation of minds, and to single out those who went by a kind of instinct to the core of the question, from those who erred in it, or who learnedly occupied themselves with its analogies, adjuncts, and details. There is no man, in my opinion, connected with the history of the subject, who has shown, in relation to it, this spirit of penetration, this force of scientific insight, more conspicuously than Sir Thomas Dick-Lauder. Two distinct mental processes are involved in its treatment. Firstly, the faithful and sufficient observation of the data; and, secondly, that higher mental process in which the constructive imagination comes into play, connecting the separate facts of observation with their common cause, and weaving them into an organic whole. In neither of these requirements did Sir Thomas Dick-Lauder fail.

Adjacent to Glen Roy is a valley called Glen Gluoy, along the sides of which ran a single shelf, or terrace, formed obviously in the same manner as the parallel roads of Glen Roy. The two shelves on the opposing sides of the glen were at precisely the same level, and Dick-Lauder wished to see whether, and how, they became united at the head of the glen. He followed the shelves into the recesses of the mountains. The bottom of the valley, as it rose, came ever nearer to them, until finally, at the head of Glen Gluoy, he reached a col, or watershed, of precisely the same elevation as the road which swept round the glen.

The correct height of this col is 1,170 feet above the sea. It is therefore 20 feet above the highest road in Glen Roy.

From this col a lateral branch-valley led towards Glen Roy. Our explorer descended from the col to the highest road in that glen, and pursued it exactly as he had pursued the road in Glen Gluoy. For a time it belted the mountain sides at a considerable height above the bottom of the valley; but this rose as he proceeded, coming ever nearer to the highest shelf, until finally he reached a col, or watershed, looking into Glen Spey, and of precisely the same elevation as the highest parallel road of Glen Roy.

He then dropped down to the lowest of these roads, and followed it towards the mouth of the glen. Its elevation above the bottom of the valley gradually increased; not because it rose, but because it remained level while the valley sloped downwards. He found this lowest road doubling round the hills at the mouth of Glen Roy, and running along the sides of the mountains which flank Glen Spean. He followed it eastward. The Spean Valley, like the others, gradually rose, and therefore gradually approached the road on the adjacent mountain-side. He came to Loch Laggan, the surface of which rose almost to the level of the road, and beyond the head of this lake he found, as in the other two cases, a col, or watershed, of exactly the same level as the single road in Glen Spean, which, it will be remembered, is a continuation of the lowest road in Glen Roy.

Here we have a series of facts of obvious significance as regards the solution of this question. The effort of the mind to form a coherent image from such facts, might be compared with the effort of the eyes to cause the pictures of the stereoscope to coalesce. For a time we exercise a certain strain, the object remaining vague and indistinct. Suddenly its various parts seem to run together, the object starting forth in clear and definite relief. Such, I take it, was the effect of his ponderings upon the mind of Sir Thomas Dick-Lauder. His solution was this: Taking all their features into account, he was convinced that water only could have produced the terraces. He saw clearly that, supposing the mouth of Glen Gluoy to be stopped by a barrier, if the water from the mountains flanking the glen were allowed to collect, it would form behind the barrier a lake, the surface of which would gradually rise until it reached the level of the col at the head of the glen. The rising would then cease; the superfluous water of Glen Gluoy discharging itself over the col into Glen Roy. As long as the barrier stopping the mouth of Glen Gluoy continued, we should have in that glen a lake at the precise level of its shelf, which lake, acting upon the loose drift of the flanking mountains, would actually form the shelf revealed by observation.

So much for Glen Gluoy. But suppose the mouth of Glen Roy also stopped by a barrier sufficiently high. Behind it, the water from the adjacent mountains would collect. The surface of the lake thus formed would gradually rise, until it had reached the level of the col which divides Glen Roy from Glen Spey. Here the rising of the lake would cease; its superabundant water being poured over the col into the valley of the Spey. This state of things would continue as long as the barrier remained at the mouth of Glen Roy. The lake thus dammed in, with its surface at the level of the highest parallel road, would act, as in Glen Gluoy, upon the friable drift overspreading the mountains, and would form the highest road or terrace of Glen Roy.

And now let us suppose the barrier to be so far removed from the mouth of Glen Roy as to establish a connection between it and the upper part of Glen Spean, while the lower part of the latter glen continued blocked up. Upper Glen Spean and Glen Roy would then be occupied by a continuous lake, the level of which would obviously be determined by the col at the head of Loch Laggan. The water in Glen Roy would sink from the level it had previously maintained, to the level of its new place of escape. This new lake-surface would correspond exactly with the lowest parallel road, and it would form that road by its action upon the drift of the adjacent mountains.

In presence of the observed facts, this solution commends itself strongly to the scientific mind. The question next occurs, What was the character of the assumed barrier which stopped the glens? There are at the present moment vast masses of detritus in certain portions of Glen Spean, and of such detritus Sir Thomas Dick-Lauder imagined his barriers to have been formed. By some unknown convulsion, this detritus had been heaped up. But, once given, and once granted that it was subsequently removed, the single road of Glen Gluoy and the highest and lowest roads of Glen Roy would be explained in a satisfactory manner.

To account for the second or middle road of Glen Roy, Sir Thomas Dick-Lauder invoked a new agency. He supposed that at a certain point in the breaking down or waste of his dam, a halt occurred, the barrier holding its ground at a particular level sufficiently long to dam a lake rising to the height of, and forming the second road. This point of weakness was at once detected by Mr. Darwin, and adduced by him as proving that the levels of the cols did not constitute an essential feature in the phenomena of the parallel roads. Though not destroyed, Sir Thomas Dick-Lauder's theory was seriously shaken by this argument, and it became a point of capital importance, if the facts permitted, to remove such source of weakness. This was done in 1847 by Mr. David Milne, now Mr. Milne-Home. On walking up Glen Roy from Roy Bridge, we pass the mouth of a lateral glen, called Glen Glaster, running eastward from Glen Roy. There is nothing in this lateral glen to attract attention, or to suggest that it could have any conspicuous influence in the production of the parallel roads. Hence, I think, the failure of Sir Thomas Dick-Lauder to notice it. But Mr. Milne-Home entered this glen, on the northern side of which the middle and lowest roads are fairly shown. The principal stream running through the glen turns at a certain point northwards and loses itself among hills too high to offer any outlet. But another branch of the glen turns to the south-east; and, following up this branch, Mr. Milne-Home reached a col, or watershed, of the precise level of the second Glen Roy Road. When the barrier blocking the glens had been so far removed as to open this col, the water in Glen Roy would sink to the level of the second road. A new lake of diminished depth would be thus formed, the surplus water of which would escape over the Glen Glaster col into Glen Spean. The margin of this new lake, acting upon the detrital matter, would form the second road. The theory of Sir Thomas Dick-Lauder, as regards the part played by the cols, was re-rivetted by this new and unexpected discovery.

I have referred to Mr. Darwin, whose powerful mind swayed for a time the convictions of the scientific world in relation to this question. His notion was—and it is a notion which very naturally presents itself—that the parallel roads were formed by the sea; that this whole region was once submerged and subsequently upheaved; that there were pauses in the process of upheaval, during which these glens constituted so many fiords, on the sides of which the parallel terraces were formed. This theory will not bear close criticism; nor is it now maintained by Mr. Darwin himself. It would not account for the sea being 20 feet higher in Glen Gluoy than in Glen Roy. It would not account for the absence of the second and third Glen Roy roads from Glen Gluoy, where the mountain flanks are quite as impressionable as in Glen Roy. It would not account for the absence of the shelves from the other mountains in the neighbourhood, all of which would have been clasped by the sea had the sea been there. Here then, and no doubt elsewhere, Mr. Darwin has shown himself to be fallible; but here, as elsewhere, he has shown himself equal to that discipline of surrender to evidence which girds his intellect with unassailable moral strength.

But, granting the significance of Sir Thomas Dick-Lauder's facts, and the reasonableness, on the whole, of the views which he has founded on them, they will not bear examination in detail. No such barriers of detritus as he assumed could have existed without leaving traces behind them; but there is no trace left. There is detritus enough in Glen Spean, but not where it is wanted. The two highest parallel roads stop abruptly at different points near the mouth of Glen Roy, but no remnant of the barrier against which they abutted is to be seen. It might be urged that the subsequent invasion of the valley by glaciers has swept the detritus away; but there have been no glaciers in these valleys since the retreat of the lakes. Professor Geike has favoured me with a drawing of the Glen Spean shelf near the entrance to Glen Trieg. The shelf forms a belt round a great mound of detritus which, had a glacier followed the formation of the shelf, must have been cleared away. Taking all the circumstances into account, you may, I think, with safety dismiss the detrital barrier as incompetent to account for the present condition of Glen Gluoy and Glen Roy.

Hypotheses in science, though apparently transcending experience, are in reality experience modified by scientific thought and pushed into an ultra experiential region. At the time that he wrote, Sir Thomas Dick-Lauder could not possibly have assigned the cause subsequently assigned for the blockage of these glens. A knowledge of the action of ancient glaciers was the necessary antecedent to the new explanation, and experience of this nature was not possessed by the distinguished writer just mentioned. The extension of Swiss glaciers far beyond their present limits was first made known by a Swiss engineeer named Venetz, who established, by the marks they had left behind, their former existence in places which they had long forsaken. The subject of glacier extension was subsequently followed up with distinguished success by Charpentier, Studer, and others. Agassiz grappled with it with characteristic vigour, extending his evidences far beyond the domain of Switzerland. He came to this country in 1840, and found in various places indubitable marks of ancient glacier action. England, Scotland, Wales, and Ireland he proved to have once given birth to glaciers. He visited Glen Roy, surveyed the surrounding neighbourhood, and pronounced, as a consequence of his investigation, the barriers which stopped the glens and produced the parallel roads to have been barriers of ice. To Mr. Jamieson, above all others, we are indebted for the thorough testing and confirmation of this theory.

And let me here say that Agassiz is only too likely to be misrated and misjudged by those who fail to grasp in their totality the motive powers invoked in scientific research. He lacked mechanical precision, but he abounded in that force and freshness of the scientific imagination which in some sciences, and probably in some stages of all sciences, are essential to the creator of knowledge. To Agassiz was given, not the art of the refiner, but the instinct of the discoverer, and the strength of the delver who brings ore from the recesses of the mine. That ore may contain its share of dross, but it also contains the precious metal which gives employment to the refiner, and without which his occupation would depart.

Let us dwell for a moment upon this subject of ancient glaciers. Under a flask containing water, in which a thermometer is immersed, is placed a Bunsen's lamp. The water is heated, reaches a temperature of 212°, and then begins to boil. The rise of the thermometer then ceases, although heat continues to be poured by the lamp into the water. What becomes of that heat? We know that it is consumed in the molecular work of vaporization. In the experiment here arranged, the steam passes from the flask through a tube into a second vessel kept at a low temperature. Here it is condensed, and indeed congealed to ice, the second vessel being plunged in a mixture cold enough to freeze the water. As a result of the process we obtain a mass of ice. That ice has an origin very antithetical to its own character. Though cold, it is the child of heat. If we removed the Bunsen lamp, there would be no steam, and if there were no steam there would be no ice. The mere cold of the mixture surrounding the second vessel would not produce ice. The cold must have the proper material to work upon; and this material—aqueous vapour—is, as we here see, the direct product of heat.

It is now, I suppose, fifteen or sixteen years since I found myself conversing with an illustrious philosopher regarding that glacial epoch which the researches of Agassiz and others had revealed. This profoundly thoughtful man was of opinion that, at a certain stage in the history of the solar system, the sun's radiation had suffered diminution, the glacial epoch being a consequence of this central chill. The celebrated French mathematician Poisson had another theory. Astronomers have shown that the solar system moves through space, and the temperature of space is a familiar conception with scientific men. It was considered probable by Poisson that our system, during its motion, had traversed portions of space of different temperatures; and that during its passage through one of the colder regions of the universe, the glacial epoch occurred. Notions such as these were more or less current not many years ago, and I therefore thought it worth while to show how incomplete they were. Suppose the temperature of our planet to be reduced, by the subsidence of solar heat, the cold of space, or any other cause, say one hundred degrees. Four-and-twenty hours of such a chill would bring down as snow nearly all the moisture of our atmosphere. But this would not produce a glacial epoch. Such an epoch would require the continuous generation of the material from which the ice of glaciers is derived. Mountain snow, the nutriment of glaciers, is derived from aqueous vapour raised mainly from the tropical ocean by the sun. The solar fire is as necessary a factor in the process as our Bunsen lamp in the experiment referred to a moment ago. Nothing is easier than to calculate the exact amount of heat expended by the sun in the production of a glacier. It would, as I have elsewhere shown,* raise a quantity of cast-iron five times the weight of the glacier not only to a white heat, but to its point of fusion. If, as I have urged elsewhere, instead of being filled with ice, the valleys of the Alps were filled with white-hot metal, of quintuple the mass of the present glaciers, it is the heat, and not the cold, that would arrest our attention and solicit our explanation. The process of glacier-making is obviously one of distillation, in which the fire of the sun which generates the vapour plays as essential a part as the cold of the mountains which condenses it.†

It was their ascription to glacier action that first gave the parallel roads of Glen Roy an interest in my eyes; and in 1867,

* "Heat a Mode of Motion," fifth edition, chap. vi.; Forms of Water, §§ 55 and 56.

† In Lyell's excellent "Principles of Geology," the remark occurs that "several writers have fallen into the strange error of supposing that the glacial period must have been one of higher mean temperature than usual." The really strange error was the forgetfulness of the fact that, in the production of glaciers, heat played quite as important a part as cold.

with a view to self-instruction, I made a solitary pilgrimage to the place, and explored pretty thoroughly the roads of the principal glen. I traced the highest road to the col dividing Glen Roy from Glen Spey, and, thanks to the civility of an Ordnance surveyor, I was enabled to inspect some of the roads with a theodolite. As stated by Pennant, the width of the roads amounts sometimes to more than twenty yards; but near the head of Glen Roy the highest road ceases to have any width, for it runs along the face of a rock, the effect of the lapping of the water on the more friable portions of the rock being perfectly distinct to this hour. My knowledge of the region was, however, far from complete, and nine years had dimmed the memory even of the portion which I had thoroughly examined. Hence my desire to see the roads once more before venturing to talk to you about them. The Easter holidays were to be devoted to this purpose; but at the last moment a telegram from Roy Bridge informed me that the roads were snowed up. I was thus thrown back upon books and memories; but these proving only a poor substitute for the flavour of facts, I resolved subsequently to make another effort to see the roads. Accordingly on Thursday fortnight, after lecturing at the Royal Institution, I packed up, and started (not this time alone) for the North. Next day at noon we found ourselves at Dalwhinnie, whence a drive of some five-and-thirty miles brought us to the excellent hostelry of Mr. Macintosh, at the mouth of Glen Roy.

We might have found the hills covered with mist, which would have wholly defeated us; but Nature was good-natured, and we had two successful working days among the hills. Guided by the excellent Ordnance map, on the Saturday morning we went up the glen, and, on reaching the stream called Allt Bhreac Achaidh, faced the hills to the west. At the watershed between Glen Roy and Glen Fintaig we bore northwards, struck the ridge above Glen Gluoy, came in view of its road, which we persistently followed as long as it continued visible. It is a feature of all the roads that they vanish before reaching the cols over which fell the waters of the lakes which formed them. One reason doubtless is that at their upper ends the lakes were shallow, and incompetent on this account to raise wavelets of any strength to act upon the mountain drift. A second reason is that they were land-locked in the higher portions and protected from the south-westerly winds, the stillness of their waters causing them to produce but a feeble impression upon the mountain sides. From Glen Gluoy we passed down Glen Turrit to Glen Roy, and through it homewards, thus accomplishing two or three-and-twenty miles of rough and honest work.

Next day we thoroughly explored Glen Glaster, following its two roads as far as they were visible. We reached the col discovered by Mr. Milne-Home, and which stands at the level of the middle road of Glen Roy. Thence we crossed southwards over the mountain *Creag Dhubh,* and examined the erratic blocks upon its sides, and the ridges and mounds of moraine matter which cumber the lower flanks of the mountain. The observations of Mr. Jamieson upon this region, including the mouth of Glen Trieg, are in the highest degree interesting. We entered Glen Spean, and continued a search begun on the evening of our arrival at Roy Bridge—the search, namely, for glacier polishings and markings. We did not find them copious, but they are indubitable. One of the proofs most convenient for reference is a great rounded rock by the road side, 1,000 yards east of the milestone marked three-quarters of a mile from Roy Bridge. Farther east other cases occur, and they leave no doubt upon the mind that Glen Spean was at one time filled by a great glacier. To the disciplined eye the aspect of the mountains is perfectly conclusive on this point; and in no position can the observer more readily and thoroughly convince himself of this than at the head of Glen Glaster. The dominant hills here are all intensely glaciated.

But the great collecting ground of the glaciers which dammed the glens and produced the parallel roads were the mountains south and west of Glen Spean. The monarch of these is Ben Nevis, 4,370 feet high. The position of Ben Nevis and his colleagues, in reference to the vapour-laden winds of the Atlantic, is a point of the first importance. It is exactly similar to that of Carrantual and the Macgillicuddy Reeks in the south-west of Ireland. These mountains are, and were, the first to encounter the south-western Atlantic winds, and the precipitation, even at present, in the neighbourhood of Killarney, is enormous. The winds, robbed of their vapour, and charged with the heat set free by its precipitation, pursue their direction obliquely across Ireland; and the effect of the drying process may be understood by comparing the rainfall at Cahirciveen with that at Portarlington. As found by Dr. Lloyd, the ratio is as 59 to 21—fifty-nine inches annually at Cahirciveen to twenty-one at Portarlington. During the glacial epoch this vapour fell as snow, and the consequence was a system of glaciers which have left traces and evidences of the most impressive character in the region of the Killarney Lakes. I have referred in other places to the great glacier which, descending from the Reeks, moved through the Black Valley, took possession of the lake-basins, and left its traces on every rock and island emergent from the waters of the upper lake. They are all conspicuously glaciated. Not in Switzerland itself do we find clearer traces of ancient glacier action.

What the Macgillicuddy Reeks did in Ireland, Ben Nevis and the adjacent mountains did, and continue to do, in Scotland. We had an example of this on the morning we quitted Roy Bridge. From the bridge westward rain fell copiously, and the roads were wet; but the precipitation ceased near Loch Laggan, whence eastward the roads were dry. Measured by the gauge, the rainfall at Fort William is 86 inches, while at Laggan it is only 46 inches annually. The difference between west and east is forcibly brought out by observations at the two ends of the Caledonian Canal. Fort William at the south-western end has, as just stated, 86 inches, while Culloden, at its north-east end, has only 24. To the researches of that able and accomplished meteorologist, Mr. Buchan, we are indebted for these and other data of the most interesting and valuable kind.

Adhering to the facts now presented to us, it is not difficult to restore in idea the process by which the glaciers of Lochaber were produced and the glens dammed by ice. When the cold of the glacial epoch began to invade the Scottish hills, the sun at the same time acting with sufficient power upon the tropical ocean, the vapours raised and drifted on to these northern mountains were more and more converted into snow. This slid down the slopes, and from every valley, strath, and corry south of Glen Spean, glaciers were poured into that glen. The two great factors here brought into play are the nutrition of the glaciers by the frozen material above, and their consumption in the milder air below. For a period supply exceeded consumption, and the ice extended, filling Glen Spean to an ever-increasing height, and abutting against the mountains to the north of that glen. But why, it may be asked, should the valleys south of Glen Spean be receptacles of ice at a time when those north of it were receptacles of water? The answer is to be found in the position and the greater elevation of the mountains south of Glen Spean. They first received the loads of moisture carried by the Atlantic winds, and not until they had been in part dried, and warmed by the liberation of their latent heat, did these winds touch the hills north of the Glen.

An instructive observation bearing upon this point is here to be noted. Had our visit been in the winter we should have found all the mountains covered; had it been in the summer we should have found the snow all gone. But happily it was at a season when the aspect of the mountains north and south of Glen Spean exhibited their relative powers as snow-collectors. Scanning the former hills from many points of view, we were hardly able to detect a fleck of snow, while heavy swaths and patches loaded the latter. Were the glacial epoch to return, the relation indicated by this observation would cause Glen Spean to be filled with glaciers from the south, while the hills and valleys on the north, visited by milder and drier winds, would remain comparatively free from ice. This flow from the south would be reinforced from the west, and as long as the supply was in excess of the consumption the glaciers would extend, the dams closing the glens increasing in height. By-and-by supply and consumption becoming approximately equal, the height of the glacier barriers would remain constant. Then, as milder weather set in, consumption would be in excess, and a retreat of the ice would be the consequence. But for a long time the conflict between supply and consumption would continue, retarding indefinitely the disappearance of the barriers, and keeping the imprisoned lakes in the northern glens. But however slow its retreat, the ice in the long run would be forced to yield. The dam at the mouth of Glen Roy, which probably entered the glen sufficiently far to block up Glen Glaster, would gradually retreat. Glen Glaster and its col being opened, the subsidence of the lake 80 feet, from the level of the highest to that of the second parallel road, would follow as a consequence. I think this the most probable course of things; but it is also possible that Glen Glaster may have been blocked by a glacier from Glen Trieg. The ice dam continuing to retreat, at length permitted Glen Roy to connect itself with upper Glen Spean. A continuous lake then filled both glens, the level of which, as already explained, was determined by the col at Makul, above the head of Loch Laggan. The last to yield was the portion of the glacier which derived nutrition from Ben Nevis, and probably also from the mountains north and south of Loch Arkaig. But it at length yielded, and the waters in the glens resumed the courses which they pursue to-day.

For the removal of the ice barriers no cataclysm is to be invoked; the gradual melting of the dam would produce the entire series of phenomena. In sinking from col to col the water would flow over a melting barrier, the surface of the imprisoned lakes not remaining sufficiently long at any particular level to produce a shelf comparable to the parallel roads. By temporary halts in the process of melting due to atmospheric conditions or to the character of the dam itself, or through local softness in the drift, small pseudo-terraces would be formed which, to the perplexity of some observers, are seen upon the flanks of the glens to-day.

In presence, then, of the fact that the barriers which stopped these glens to a height, it may be, of 1,500 feet above the bottom of Glen Spean, have dissolved and left not a wreck behind; in presence of the fact, insisted on by Professor Geikie, that barriers of detritus would undoubtedly have been able to maintain themselves had they ever been there; in presence of the fact that great glaciers once most certainly filled these valleys—that the whole region, as proved by Mr. Jamieson, is filled with the traces of their action ; the theory which ascribes the parallel roads to lakes dammed by barriers of ice has, in my opinion, an amount of probability on its side which amounts to a practical demonstration of its truth.

Into the details of the terrace formation I do not enter. Mr. Darwin and Mr. Jamieson on the one side, and Sir John Lubbock on the other, deal with true causes. The terraces, no doubt, are due in part to the descending drift arrested by the water, and in part to the fretting of the wavelets, and the rearrangement of the stirred detritus, along the belts of contact of lake and hill. The descent of matter must have been frequent when the drift was unbound by the rootlets which hold it together now. In some cases, it may be remarked, the visibility of the roads is materially exalted by differences of vegetation. The grass upon the terraces is not always of the same character as that above and below them, while on heather-covered hills the absence of the dark shrub from the roads greatly enhances their conspicuousness.

Reviewing our work, we find three considerable steps to have marked the solution of the problem of the Parallel Roads of Glen Roy. The first of these was taken by Sir Thomas Dick-Lauder, the second was the pregnant conception of Agassiz regarding glacier action, and the third was the testing and verification of this conception by the very thorough researches of Mr. Jamieson.* To these may be added the important observation of Mr. Milne-Home in Glen Glaster ; with other remarks and reflections scattered through the literature of the subject, or suggested by the latest visit to the spot.

Thus ends our rapid survey of this brief episode in the physical history of the Scottish hills—brief, that is to say, in comparison with the immeasurable lapses of time through which, to produce its varied structure and appearances, our planet must have passed. In the survey of such a field two things are specially worthy to be taken into account—the widening of the intellectual horizon and the reaction of expanding knowledge upon the

* No circumstance, or incident, connected with this discourse gives me greater pleasure than the recognition of the value of these researches. They are marked throughout by unflagging industry, by novelty and acuteness of observation, and by reasoning power of a high and varied kind. These pages had been returned "for press" when I learned that the relation of Ben Nevis and his colleagues to the vapour-laden winds of the Atlantic had not escaped Mr. Jamieson. To him obviously the exploration of Lochaber, and the development of the theory of the Parallel Roads, has been a labour of love.

intellectual organ itself. At first, as in the case of ancient glaciers, through sheer want of capacity, the mind refuses to take in revealed facts. But by degrees the steady contemplation of these facts so strengthens and expands the intellectual powers, that where truth once could not find an entrance it eventually finds a home.—*A Lecture delivered before the Royal Institution, June* 9, 1876.

WHAT IS THE MEANING OF HUMAN PERSONALITY?

By HENRY J. SLACK, F.G.S., Sec. R.M.S.

WHAT constitutes human personality? It is a kind of consciousness, complex in character. Memory is concerned in it, or we should not associate the past sufficiently with the present, or with the closely recent, to supply the means of forming the conception of continuity. A mere reproduction by memory of the image of the past would not be sufficient to aid us in this matter; for, as Dr. Carpenter observes, "there must be a recognition of the reproduced state of consciousness as one which has been formerly experienced; and this involves a distinct mental state, which has been termed the 'consciousness of agreement.' Without this recognition we should live in the present alone."

Thus a consciousness of existing *now* combined with this "consciousness of agreement," as relates to the past conditions which memory recalls, are necessary to the idea, or sensation of personality, as it exists in man unimpaired by disease. A lower kind of personality might comprehend present existence only; but if a creature so constituted were able to profit by the results of experience, it would be as automatically as if it were a machine constructed to modify its action after receiving certain impulses. It would not be capable of human *experience*, that is of experience in the sense in which the word is usually employed to designate an impression that was once a subject of consciousness and its conscious recollection.

Dugald Stewart remarked that "we cannot properly be said to be conscious of our existence, our knowledge of this fact being necessarily posterior in the order of time to the consciousness of those sensations by which it is suggested." The time occupied in the transmission of an impression from, say a finger, with its sense of touch, to the sensorium, and its conversion into a sensation of resistance, hardness, softness, smoothness, or roughness, is a minute fraction of a second. Nerve action, like electrical action, appears to consist in pulsations, and the *personality* of the supposed simpler creature with the lower personality would consist in intermittent productions of consciousness. But, if personality is a matter of consciousness, and is thus distinguished from the mere individual or distinct existence belonging to minerals, such a being would be, and not be, a person, in alternations, like the beats of a clock. The unconscious intervals might be long or short; the sensation would be the same. It is conceivable that beings might exist whose pulsations of consciousness and personality varied from millionths of a second to millions of years. If able to think, and satisfied with Descartes' maxim, *cogito ergo sum*—"I think therefore I am"—one such would be certified of its existence through either alternating millionths of a second or millions of years.

Dr. Carpenter's view of personality involves a series of nervous actions, each of which may be regarded as pulsations; and we have no reason to affirm that the rate of our nerve action, or pulsation, is the only one that can answer the purpose of producing a sense of personality bearing analogy to our own. We set our clocks by the beats of seconds; but if we lived on the sun, and had sight of the central sun round which he is supposed to move and carry his attendant worlds in millions of years, we might substitute centuries for seconds as our time units, and perhaps could do well with nerve pulsations proportionably long in their intermittent intervals. At each beat we should be persons, in the intervals no persons; but memory and consciousness would tie together the periods of personality, and those of no personality could be known only as inferences which knowledge might enable us to draw.

If personality, such as Dr. Carpenter defines it, could exist in all the stages of insect life—egg, grub, chrysalis, butterfly—the sense of continuity would be handed on through quite different states of existence. In the case of man, his healthiest normal personality runs through periods in which he develops, or changes, within limits that are much narrower, and which cause him to retain one character, or nature, throughout. In many cases of insanity, however, the change of character is so great that the afflicted person may be said to have become some one else. "A man beside himself" is a well-known phrase, intimating the sort of dual being that passion, or disease, or narcotics, may make of what should be an orderly uniform being.

The perceptive, the intellectual, and the moral faculties, are all subject to modification from cerebral disturbance, or disease. In some cases the sufferer is conscious of his errors, or delusions; and a well-known physician, having the care of the insane, informs the writer that hopes and prospects of cure are then much stronger than when the patient cannot be persuaded there is anything wrong with him. In these instances there is a kind of double personality, one surveying the other, and knowing it to be wrong-headed. When the errors, or delusions, are not recognised, the personality is single, but different from that which existed in a sound state. When insane people of normally good characters fancy they have committed crimes, and feel horror and remorse, the character, or personality, remains the same, but is the victim of delusion. From this mode of speaking it must not be supposed that character and personality are regarded as the *same* thing; but what is called *character* consists in attributes of the personality, and when those attributes suffer a great change, the result is like a transformation of one person into another. Thus, in a case mentioned by Forbes Winslow, on the authority of Dr. Brierre de Boismont, a person in high office, who had performed the duties of his station satisfactorily, and in private life exhibited generosity and honesty, became mean, avaricious, licentious, and fraudulent. Similar disorders cause "the brave and heroic to become as timid and bashful as any maiden in particular states of ill health. Mild, inoffensive, and humane men are driven to acts of desperation and cruelty;" and modest girls indulge in indelicate actions and disgusting talk. The late Forbes Winslow thought such outbursts of evil came from the "innate corruption and natural depravity of the human heart"; but scientific men do not allow theological crotchets to serve as explanations for physical facts; and although no one can afford the slightest explanation of why and how thought and emotion are connected with chemical and molecular changes in nervous or cerebral matter, abundant cases prove that physical disturbance by disease or wounds can produce the changes of character which we are now considering. Not only can mechanical violence change character in certain cases for the worse, but we find opposite instances recorded where there has been a beneficial result. Thus, to cite an instance from Forbes Winslow's unphilosophical but amusing and useful story-book, "Obscure Diseases of the Brain and Mind,"—"a child, up to the age of thirteen idiotic, giving evidence either of a total deficiency of intelligence or of a stunted intellect of the lowest grade and order, fell from a height upon his head, and was stunned. He rallied from this state of unconsciousness, and was, *credat Judæus*! found to be in full possession of his intellectual faculties." Father Mabillon is also said to have been cured of idiocy, at the age of twenty-six, by tumbling against a stone staircase and fracturing his skull, for which he was trepanned, and thereupon exhibited a "lively imagination, an amazing memory, and a zeal for study unequalled." In another instance a soldier's intellect was improved by a musket-ball knocking out some of his brains.

In the two cases just mentioned, the personality of the boy and that of the priest was changed from an idiotic one to a rational one by a physical accident; and it is interesting to compare such instances with those of the opposite sort, where, instead of the new and induced character being better, it has been horribly worse.

When persons were observed to suffer under that kind of dualism in which their proper selves were tempted and tormented by the second and morbid self, we cannot wonder that our forefathers, ignorant of science, thought them subjects of demoniacal possession. There seemed to be two distinct persons in one body; and many of the old ascetics, who, like modern lunatics, knocked their heads against stones, and tormented their flesh, acted under the notion that their real personality—that which they thought themselves—had to fight with a hostile and diabolical personality which occupied and used their bodies. When the delusions are constant, and the original man struggles against them, the dual personality is complete and continuous; but cases frequently occur of what is called "double consciousness," in which there is a dual personality, each one of which has a separate period to itself. Many of these are related in well-known books, but the following is taken from a recent number of the "Revue Scientifique" (May 20).

It is recorded by M. Azam, from whose dreadfully prolix narrative we glean the following facts:—The subject of the disorder is Felida X., born of healthy parents at Bordeaux in 1843. Her father, a merchant captain, died while she was young, leaving a family for the mother to support. Thus Felida's early years were spent in poverty, but she developed in the usual way till about thirteen, when, just after puberty, she exhibited hysterical symptoms, accompanied with pulmonary hæmorrhage, without apparent cause from the condition of the respiratory organs. About fourteen she suffered sharp pains in both temples and fell suddenly into an unconscious state like sleep, which lasted about ten minutes, after which she opened her eyes and entered into what M. Azam calls her second state. This lasted an hour or two, after which the fainting and sleep reappeared, and she returned to what the doctor calls her ordinary state. More attacks came on every five or six days, or less often; and her relations, noticing a change in her condition during the second life, and her forgetfulness of it on awakening, thought her idiotic. The hysteria got worse, and she suffered convulsions, which led to M. Azam, who was connected with a lunatic asylum, being called in. This was in 1858, and he found Felida a plump brunette of moderate height, very intelligent for her position in life, but melancholy, morose and taciturn, with a strong will, and very industrious at needlework. She suffered from frequent hæmatopsies, complained of pains in the head, and hysterical symptoms. She was very anxious about her condition. Her acts and conversations showed no intellectual defect, but her affective sentiments were feebly developed. Nearly every day, without any perceptible cause, she suffered what she called her "crisis," and entered into the second state. All of a sudden, with her work on her knees, a violent pain shot through her temples, her head dropped upon her breast, her arms fell by her side, and she passed into a sort of sleep from which neither noises, pinches, nor pricks could awaken her. This lasted two or three minutes, but had formerly been longer. She woke up in quite another state, smiling gaily, speaking briskly, and trilling (*frédonnant*) over her work, which she recommenced at the point she left it. She would get up, walk actively, and scarcely complained of any of the pains she had suffered so severely a few minutes before. She busied herself about the house, paid calls, and behaved like a healthy young girl of her age. In this state she remembered perfectly all that had happened in her two conditions. In this second life, as in the other, her moral and intellectual faculties, though different, were incontestably sound. After a time, which in 1858 lasted three or four hours, the gaiety disappeared, the torpor suddenly ensued, and in two or three minutes she opened her eyes and re-entered her ordinary life, resuming any work she was engaged in just where she left off. In this state she bemoaned her condition, and was quite unconscious of what had passed in the previous state. If asked to continue a ballad she had been singing, she knew nothing about it, and if she had received a visitor she believed she had seen no one. The forgetfulness extended to everything which happened during her second state, and not to any ideas or information acquired before her illness. In 1858 her hysterical condition was well characterised, and in what M. Azam calls her "normal state" she could not taste nauseous pills; her sense of smell was deadened, and many points on her body were without sensation. The least emotion brought on convulsions without complete loss of consciousness. At this period occurred what M. Azam calls an "epiphenomenon" of the attack, which he saw only two or three times, and which a young man she married only saw thirty times in sixteen years. Instead of waking as usual from her second state, she did it in a fit of terror, and recognised no one but her husband. This did not last long, and it was only on such occasions that anything like hallucination was observed. Her two mental conditions were strikingly exhibited in reference to an incident of courtship preceding her marriage to a young man she had known from childhood. One day, when more than usually sad, she told the doctor she was getting worse, that she was enlarging, and suffered from nausea each morning, with other symptoms of conception. After the next attack, which came soon, she said: "I remember perfectly what I have just told you, and I now confess I am with child." She had got into this condition in her second state, and was quite ignorant of it in her normal state; but this was rudely broken through by a neighbour, who thought she was shamming innocence, and told her what she had confessed in the other state. The shock to her feelings brought on violent hysterical convulsions, which required medical attention for two or three hours. Her first child was born when she was seventeen and a half, and for the two following years she enjoyed good health. When about nineteen and a half the attacks recurred with moderate intensity, and a year later she had a difficult confinement, blood spitting, and a lethargy which lasted three or four hours. From this time till she was twenty-four the attacks became more frequent, and their duration, which at first equalled the normal periods, began to exceed them. The pulmonary hæmorrhages increased, and she suffered partial paralyses, lethargies, extasies, &c. For three years, from twenty-four to twenty-seven, she was entirely in the normal state, and then for another six years, up to 1875, the disorder reappeared. Up to this date she had gone through eleven confinements, the result being two children still living. The eldest, born during one of the attacks, is nervous like his mother, and suffers from fits of terror. He is intelligent and a good musician, now sixteen years old. M. Azam gives numerous instances of the two states, and adds diagrammatic representations of them, one of which traces their changes of duration. From fourteen and a half to seventeen and a half years of age the periods of *accès*, or attack, were far between and much shorter than the normal states. From seventeen and a half to twenty and a half the normal state lasted; from twenty and a half to twenty-four three attacks; from twenty-four to twenty-seven a long normal state; from twenty-seven to thirty-two increasing *accès* states, and at the present date these states tend to become constant. M. Azam observes that "the growing diminution in number and length of the normal states leads to the opinion that they may at last cease altogether, and then her whole life would be passed in the second state." She would then have a complete personality; intelligence and memory would be there, but it would not be the old personality; she would be another person:—

"Her existence, from an external view, would include three successive personalities; the first normal, with which she came into the world; the second, divided in two by loss of memory (*amnésie*); the third new, and wholly different. Good would thus come from excess of evil, for it would end in a sort of cure. I dare expect no other, and if it come it will be in twelve to fifteen years, at a critical age."

It is a pity that this story is not told with more completeness; for, although nothing of much importance is omitted in the preceding condensation of the original account, it leaves us without many facts which should have been recorded. After the first hysterical attack, the girl appears to have had two personalities strongly marked by differences of character. In one state she was morose and melancholy; in the other lively and agreeable. Was this state like her original condition before puberty, allowing for the usual development between one age and another? Dr. Azam gives us no information on this point. He tells us that "in her second life her moral and intellectual faculties, though different, were incontestably sound." But it was in this state she became too familiar with the young man whom she subsequently married—a transaction of which she had no cognizance in what the doctor terms her "normal" state, misusing that word, which he subsequently employs with more propriety to designate that condition in which she came into the world, and in which, while it lasted, she behaved like other children.

The second state, the birth one being her first, was that of an intermittent personality; the third of two personalities, composed of the second, and a new one, occurring in alternations, and linked together by those actions of memory and consciousness described at the beginning of this paper in the words of Dr. Carpenter.

Should a cure ultimately ensue, as Dr. Azam expects, her final state would apparently be a reversion to her birth state, with such modifications as naturally belong to lapse of time. Suppose, instead of an act of impropriety during the second state, she had committed a robbery or an assassination, no moral responsibility could have been assumed to rest upon her with any certainty by any one acquainted with her history; though without a knowledge of the previous facts no excuse would have appeared.

In these double consciousness cases the original personality is only lost at intervals; but when, in confirmed insanity, a patient supposes himself quite another character, and assumes the thoughts and feelings considered proper to such a character, the original personality, as an essential property, disappears altogether. Temporary disappearances of the real personality and its replacement by an assumed one, not only occurs in cases of insanity, but can be induced by the action on the nervous system of what has been termed "electro-biology." Thus Dr. Carpenter records having seen a lady "metamorphosed into the worthy clergyman on whose ministry she attended, and with whom she was personally intimate." He says "he shall never forget the intensity of the lackadaisical tone in which she replied to the matrimonial counsels of the physician to whom he (she) had been led to give a long detail of his (her) hypochondriacal symptoms: 'A wife for a dying man, doctor!' No *intentional* simulation could have approached the exactness of the imitation, alike in tone, manners, and language, which spontaneously proceeded from the idea with which the fair 'subject' was possessed, that she herself experienced all the discomforts whose detail she had doubtless frequently heard from the real sufferer." *

While this state lasted the lady's own personality vanished, and she was, so far as her own knowledge and consciousness were concerned, the hypochondriacal clergyman whose ways she imitated. In a case of intoxication the loss of the real personality was only partial—a man knew perfectly well who he was, but referred all his own drunken symptoms to his family, and insisted upon undressing them and putting them to bed, affirming that they had taken too much to do those things for themselves!

The alternating form of dual personality may possibly explain some of the stories of "spiritual manifestations." A believer in these performances cannot be induced to accept as a solution of the apparent wonders the explanation that the medium is cheating him. The said medium, male or female. as the case may be, is well known, he says, to be a most veracious person, quite incapable of deceit—a most simple-minded person, unable to concoct a fraud. This may be so in the normal condition, but what has the so-called mediumistic state done? It usually comes on, more or less, like the somnolence of the girl Felida, and perhaps it transforms a reliable person into either an unwilling dupe of morbid sensations, or a cunning cheat.

Cerebral physiology is not yet advanced enough to say how the brain acts, or whether functions are rigidly localised. Professor Golz has recently shown that functions which seem quite destroyed by excision of large portions of one hemisphere, reappear after a time, if the animal subjected to the process can be kept alive. A physiologist who contributes to the "Academy," referring to these experiments as narrated in "Pflüger's Archives," says: "A belief in the existence of localized centres in the cortical substance is incompatible with the fact that lesion of any part whatever of a hemisphere is followed by one and the same train of symptoms, and with the observed restoration of the particular functions over which those centres are supposed to preside." May one who has no pretensions to be a physiologist suggest that the grey matter of the brain may have its

* "Mental Physiology," p. 523.

molecules arranged in patterns somewhat analogous to those of steel-filings under the influences of a magnet, but that in someway the direction of the forces—or vibrations—may be changed in them. The pattern will then be different, and the position of supposed organs altered. If this be true, the search should not be for organs, but for centres of action, which in healthy brains may have fixed positions.

Leaving this and other guesses for what they are worth, we must admit that personality as we know it is a result of organisation, and that a molecular change, or a variation in the rate, or character, of those chemical actions and decompositions that are the invariable physical antecedents of thought, sensation, or volition, can instantly convert one personality into another, in which that sense of continuity which links the personality of yesterday with that of to-day may be wholly or partially destroyed. Thus we may see realised something like Circe's magical transformation of men into beasts, or a new man created surpassing the old.

Many will be startled at the notion of their personality being intermittent, not continuous; but if it consists in a series of impressions converted into consciousness, with the memory's links tying them together, the sense of the continuity of our existence, which we all feel, must be like the sensation of a continuous sound produced by successive beats at small intervals, or of continuous light from rapidly recurring impressions. If our modes of consciousness enabled us to take note of infinitesimally minute time intervals, and of infinitesimally minute molecular changes, each second would be crowded with a corresponding infinity of impressions, and a personality lasting a few minutes would be more amply filled with incidents and recollections than a human life of the longest term of years. If, with our present limitations of power to perceive minute time intervals, we were able to notice the molecular changes that took place between one pulsation of personal consciousness and another, we should suppose that nature, contrary to the old adage, did act by jumps. If a being could exist with enormously slower pulsations of conscious existence, these jumps would seem enormous, and, in an extreme case of such a supposition, the world that existed in one period might be extinguished and rearranged before another came on. The whole philosophy of such a being would differ from ours.

But if intermittence and change be the condition of our lives, is there nothing permanent to which we can cling? Physical Science has to do, in its present stage, only with facts belonging to the regions of incessant change; but man must, from his constitution, form an ideal of the continuous and the enduring towards which he aspires, and in which, in spite of doubts, he in the long run believes.

THE VIVISECTION CLAMOUR.

By the EDITOR.

HAS the outcry ceased now that a Bill has been passed by the House of Commons? We fancy not. The Bill has not satisfied those who are opposed to vivisection, while of course it has not pleased the medical profession. But the voice of the opposition has not been as loud as in the beginning of the year. Then it was shockingly violent and unreasonable. There has been, as the old saying is, "much cry and little wool," and a number of well-intentioned and equally uninformed old women have raised a banner with "cruelty to animals" inscribed upon it. This, of course, has been followed by an ignorant and withal a very noisy crowd, who have raised a special outcry against the medical profession, of abhorrent cruelty to animals. The medical profession at first took little notice of this babbling crowd; and we fear that if it had not been for the decided movement of Mr. Ernest Hart it would have been absolutely silent on the matter, and as a consequence we should have had the Act passed in its original form, which has, now that its intentions are familiar, become so objectionable to the entire profession. Mr. Hart saw the objectionable character of the proposed law, and he called public attention to the subject through his paper, the "British Medical Journal," and by a well-organised plan he got the profession together, and brought them in a body to Mr. Cross to protest against this measure (as it was then drawn up) becoming law. The occurrences at that meeting are familiar to the public. Mr. Simon and Sir W. Jenner pointed out, in terms of the most clearly logical force, the utterly unfair nature of the Bill, and the slur which it would cast upon what is unquestionably the most charitable profession in existence. While at the same time, as Mr. Simon indicated at some length, the profession of medicine is charged with cruelty—cruelty whose committal is under chloroform—the fox hunter, the harrier keeper, the deer stalker, the pigeon slayer, the rabbit snarer, to mention but a few,* are allowed to perpetrate the utmost cruelties imaginable without aim or end other than the unquestionable torture of the animals pursued. A calm observer of the arguments on the two sides will unavoidably say, "If you desire to put down cruelty to animals, put it down first where it is both harsh and unnecessary; and then if you will, come down on vivisection," † which is an extremely useful as well as excessively limited operation—profession, indeed, we had almost termed it.

And this view of the matter brings us to the consideration of some remarks on this subject made by the "Daily News" of August 10 or 11. This paper, in a leading article on the discussion in Parliament upon the second reading of the Bill, holds the same opinion, but it singularly enough commences at the end instead of at the beginning. It says that of course Parliament cannot undertake to legislate for the whole matter at once, but that it does well to begin with the physiologists. Why? Simply that the "Daily News" has got a craze upon the subject, and takes this unreasonable view. Surely if vivisection is to be put down at all, *i.e.* if all modes of torturing creatures are to be put an end to—an idea which is as absurd as it is impossible—that which is most general and least useful should in the first instance be considered. But, in the opinion of the "Daily News," the reverse view is correct. That which is most beneficial and infinitesimally small in its amount must be the first that is put an end to.

Apart, however, from this view of the question, there is a series of other points to be considered with which we fear the anti-vivisectionist has not at all acquainted himself. And firstly comes the fact that every animal must die at one time or another. It may be a slow death by starvation, it may be of the most intense torture—as witness a cat with a mouse or rat which she has half killed and then plays with for hours; it may be by poison—which is essential in many cases; or, lastly, it may be by some sudden catastrophe—as a flood, or a fire, or frost which slays by thousands. And to this the anti-vivisectionist will reply: But why not allow animals to live as long as they can? To this important question two answers are to be given. 1. A lower animal differs entirely from man, inasmuch as it never knows that it is going to die. If the condition of man's life were the same, death could have no earthly horror, *i.e.* death alone. If,

* Not to mention the habit of puncturing animals with iron goads for the purpose of marking them with the owners' initials, the crimping of live salmon and cod, the amputation of sheep's tails, &c. &c.

† The word should never have been used, and yet it seems difficult to substitute a better one. But the idea that it conveys of animals being flayed alive is clearly as objectionable as it is wrong.

then, we kill an animal suddenly, without allowing it to experience the sense of pain, how have we been guilty of cruelty? It has no knowledge of the intention we have had of killing it; it has the most perfect enjoyment of life to the last moment. It knows not of our having anything to do with its death. In fact, it is killed without having had an idea of mortality. Then what is the injury inflicted? 2. But there is another and vastly more important argument. This is the one which is furnished in the most convincing form in Mr. Darwin's "Origin of Species." It is in four words, "The struggle for life." I suppose my anti-vivisectionist friends are unaware of the fact, but nevertheless fact it is, that if all the animals that were brought into the world during the last fifty years were allowed to live for their natural term of years or months, as the case may be, the country would be over-run, there would not be standing room in all probability for one of us. Living would be utterly out of the question, if such a condition existed, for the lower animals alone.

Let us take a few well-known examples from Mr. Darwin's book in proof of this statement. In the sixth edition (the last but one), p. 50, of his "Origin of Species," the author gives the following example of the geometrical ratio of increase, which shows us clearly enough that for one animal of any kind that lives, thousands perish before reaching maturity; and that were it not so, the world would become uninhabitable:—

"A struggle for existence inevitably follows from the high rate at which all organic beings tend to increase. Every being which during its natural lifetime produces several eggs or seeds must suffer destruction during some period of its life, and during some season or occasional year. *Otherwise, on the principle of geometrical increase, its numbers would quickly become so inordinately great that no country could support the product. Hence, as more individuals are produced than can possibly survive, there must in every case be a struggle for existence.* . . . . It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms. . . . . Although some species may be now increasing more or less rapidly in numbers, all cannot do so, for the world would not hold them."

"There is no exception to the rule that *every organic being naturally increases at so high a rate that if not destroyed the earth would soon be covered with the progeny of a single pair.* Even slow-breeding man has doubled in twenty-five years; and at this rate in less than a thousand years there would literally *not be standing-room for his progeny.* . . . The elephant is reckoned the slowest breeder of all known animals, and I have taken some pains to estimate its probable minimum rate of natural increase. It will be safest to assume that it begins breeding when thirty years old, and goes on breeding till ninety years old, bringing forth six young in the interval, and surviving till 100 years old. If this be so, after a period of from 740 to 750 years, there would be nearly *nineteen million elephants alive descended from the first pair.*"

Now, the above remarks—and they are but a tithe of those that could be quoted, and which we trust the reader will look out for himself—clearly prove that the amount of cruel slaughter which takes place wholesale in Nature, and which is really for the benefit of us and the other few who survive in the struggle for existence, is of so vast a character that anything in the nature of vivisection sinks beside it into such utter insignificance as to be totally unworthy of observation by the philosopher.

But if we leave these grand aspects of the question, and for a moment turn the argument against the anti-vivisectionists, what shall we see? Why, that they who cry out loudest are themselves guilty of the most intense cruelty. Which of them for instance, will avoid eating a chop, and yet what is that but the product of cruelty to animals. The poor little beast first has its tail cut off when it is a young animal. Then its testicles—if it is a male—are cruelly dissected out, *without* the influence of chloroform; and then when it has reached a fair degree of size and fatness it is killed. And if we inquire whether this act is performed under chloroform, the mere question excites a smile. Again, if it is a calf that is killed, how is the act performed? Why, most cruelly. Veal is a sort of meat which the public like to have well-bled; it is a so-called white meat. Well, the unhappy little calf is torturingly allowed to bleed to death. And how many millions of these creatures have to suffer in this manner every year! Again, will the anti-vivisectionist not destroy without hesitation all bugs, fleas, lice? Will he not, without the faintest scruple, remove and slay with a ruthless hand thousands, nay millions, of aphides; and will he not squash beneath his feet the slug and snail that inhabit his garden? Yet surely he will not tell us that creatures like *Aphis*, *Limax* and *Helix* are devoid of sensation—the latter, indeed, endowed with the most complete nervous system. Or will he not hunt the fox, or snare the rabbit, or eat his chicken that has been cruelly bled to death by the poultry-man, who places its body between his knees and slashes a huge knife across its neck, and waits, without the slightest scruple of conscience, till the fowl has bled to death? Or is he ignorant of those choice morsels of delicacy known as the *pâté de fois gras?* Does he know what torture these poor birds are put to through their whole lives in order that the taste of the *bon vivant* may be sufficiently pleased? Or does he eat his lobster or crab, and does he know how they are prepared? Is he ignorant of the fact that they are kept out of water for hours, which has somewhat the same effect as though he were being submerged for half-a-minute say every five minutes in the twenty-four hours. But that is not all. A cauldron of boiling, steaming water is ready, and into this the still living crab or lobster, shrimp or prawn, is at once plunged. Just conceive for a moment the agony the poor brute must suffer!

Better still, let him reflect on his oyster-eating habits. A poor oyster is at least a couple of days out of his native element. Still he has sufficient liquid floating about his gills—and how beautiful these gills are the microscopist alone knows—to keep him alive till the moment that anti-vivisectionist begins his dinner or supper. Then is a knife passed through his gills and across his muscles that close the valves; next for a few seconds he is tortured by having some burning compound poured—as oil upon his wounds—upon him, and, finally, in a moment, while still in a dying state, is he plunged into the mouth and stomach of the man or woman who grieves so loudly over vivisection.

Is it not wonderful in this, which is especially the Age of Reason, to see so much of absolutely arrant nonsense talked by people whom we should take in other respects for sensible persons. Christ's argument is the most potent one in this case, and well may the vivisectionist say, "Let him that is without sin among you cast the first stone." Assuredly if they are anywhere, the Pharisees are among our opponents.

If we wanted further evidence of cruelty of the most painful kind, but which is yet absolutely unavoidable, we have only to go to any of our sea-side fishing villages. There we can see whole boat-loads of crabs slowly dying, which are torn limb from limb in the still existing condition to form bait for the fishermen. Again, look at the millions—nay, the billions—of herrings, pilchards, not to mention other salt-water fish that are caught on our own coasts year after year. Are they chloroformed or even killed at once? Assuredly not; they are allowed slowly to die of suffocation, not sudden, as in the case of a man who is drowning, but gradual suffocation, which takes some hours to complete. Then let us take fresh-water fish. Is not the salmon, if he is caught on the line, sometimes put to torture for half-an-hour or more, and is he not then most cruelly dealt with—a large, bent spear, the gaff it is called, is driven through his body and thus he is brought to land? Again, the trout, or perch, or roach, &c., is he not taken with a hook which pierces his mouth, and is he not then allowed to die the death of suffocation and pain in the basket of the angler? Take the practice which exists in some rivers of fluking. A man lying forward in the bow of a boat floating over a sandy bottom, sees the fluke—the young plaice or sole—buried in the sand, then he thrusts his spear through the animal's back, taking care that it penetrates its body completely. Next he lifts the weapon, disengages the fluke by withdrawing the spear from its body, and allowing the unhappy victim to flounder to the bottom of the boat in most excessive agony, permits it to die, which it generally does in about four hours; while he goes on spearing in most cases till he has five or six dozen of these flat-fish captured. Then, most of them still alive, they are strung on a switch of willow, which is passed through their gills and out of their mouth, and so are conveyed for miles to market.

If the above is not cruelty, we know not what is. But it might be multiplied almost infinitesimally, if we were to obtain a return of the actual amount perpetrated throughout the country.

And if now we compare with these several modes of cruelty, which are performed beneath the eyes of all, the methods of vivisection as they are practised in this country, what shall we see? We first of all find that there are in the whole land not more than from twenty to thirty persons who have to do with physiological experiments which demand operations on animals. And again, we may suppose that they work, say ten months a year, and we will further suppose that on an average they have an operation to be performed on a mammalian animal about three times a week. We are now drawing a fair estimate. Well, and what is the cruelty? The animal is tied up and in the first instance is placed under the influence of chloroform. Then possibly—it being incapable of feeling the slightest pain—a knife is inserted in its abdomen and a bit of its intestine is tied, or its skull is partly removed to see the condition of its brain or to try experiments with galvanism; or its heart or lungs are exposed for the purposes of conducting some experiment on circulation or respiration; or it may be the absorption of fat by the lacteals that the experimenter wishes to determine. Well, the animal is kept under chloroform, save in some very special cases, during the operation. Then if the injury is very slight it is allowed to recover, if the destruction of tissues has been great it is, while still under chloroform, put an end to by the simple process of "pithing."

And such cases as I have given an instance of, are, as we have seen, but few indeed when performed on the higher animals, as dogs and cats. Guinea-pigs, frogs, and newts constitute the mass of the experimenter's subjects, and we can say that there is never an example of the intense cruelty that many of the anti-vivisectionists have spoken of. Indeed, students are exceptionally considerate to the subjects under examination, as we doubt not those who have had to perform experiments on animals will bear us out in testifying.

But if we have not a sufficient argument in favour of vivisection from the tendency to spare the feelings of animals by the use of chloroform on the part of the vivisectors, and from the fact that vivisection is as to cruelties practised every day through stupidity and sport (! ! !) in the ratio of about 1 to 1,000,000, we have a last argument of greater import than any that have gone before. It is that vivisection is of use, and that it is by means of vivisection that nearly all those results have been attained which have led to establish Medicine on a partly rational basis, and which have led to advances in the art of medicine—in its widest sense—that have immensely tended to the relief of human suffering. It would, of course, be out of our power in such an article as this to give a list of the series of experiments on animals that have led to the correct ideas on the subject of digestion, circulation, respiration, absorption, secretion, and more recently on muscular action and brain-power. But those who are interested in this important question will do well to refer to a valuable article in the "British Medical Journal" of the present year, in which they will find the whole of this question amply, and fairly, and dispassionately discussed.

Is it not, then, unfair as well as unwise to present a Bill on this subject which deals exclusively with the medical profession. We think it is; yet it has been done. Mr. Cross has succeeded in getting a Bill passed through the House almost at the last moment, just as the members might be said to have been looking out their guns and cartridges, and preparing for the terrible slaughter of the twelfth. Just at this moment they have passed a Bill to hamper and impede the labours of the practical physiologist. What a magnificent subject for a clever satire. And yet, though the Bill is objectionable to the medical profession in some of its features, more especially so in its prevention of vivisection in any medical school, and in its application to vertebrate instead of warm-blooded amimals, the anti-vivisectionists are outrageous with Mr. Cross for his timidity (! !) in leaning towards the medical profession. Thus has the Home Secretary fallen into the position usually occupied by the person who sits between two stools.

We think that the only member of the House of Commons who spoke rationally and calmly on the subject was Mr. Robert Lowe, and we thoroughly sympathise with all the observations he made upon the subject.

The anti-vivisectionists are dissatisfied, and mean to open the battle again next session. But things will be changed then, and we doubt not that much of the wild frenzy so often associated with the petticoat will have had time to cool down, so that altogether while we do not approve of the form legislation has taken, we are yet hopeful as to the future, and we would urge upon our medical readers to use all their influence with their parliamentary friends towards the utter abolition of this Bill in the next non-Disraelian session of Parliament.

REVIEWS.

GEOGRAPHICAL DISTRIBUTION OF ANIMALS.*

TO review a work like the present one would be, except to some half-dozen writers in the same field, an absurdity, if not an impertinence. To give even an analysis of its contents would require infinitely more space than that of our entire "Reviews." What can we do, then, in the present instance? We can just briefly give an account of the nature of the volume, of what it is the author has attempted to do, and of the time and space which his labours have occupied, and their results have been embodied in. We may say that to us Mr. Wallace appears to have very faithfully succeeded in doing what he wished, viz. that his "book should bear a similar relation to the eleventh and twelfth chapters of the 'Origin of Species' as Mr. Darwin's 'Animals and Plants under Domestication' does to the first chapter of that work." In this respect we may say that the analogy is complete. Nay, more; we fancy that Mr. Wallace's labours, as put forward in the two splendid volumes now before us, will be read when Mr. Darwin's volumes on "Animals and Plants" will have fallen into desuetude. Whether it will have as long a lease of existence as the "Origin of Species," it is, of course, impossible to say; but of its great importance as a scientific treatise there cannot be the least doubt in the mind of anyone who is acquainted with the history of Nature; and this all the more so because it is a work *sui generis*. Mr. Wallace has had to undertake an entirely novel labour in preparing those volumes, and the result is the production of a work that is completely new. Not only so, but it is executed by one who is a thorough master of the subject; by the man who might, but for his extreme modesty, be now in the proud position which Mr. Darwin holds—that of the first naturalist in the world. It is clear, therefore, that we may anticipate a subject dealt with in a philosophic spirit, as such a subject especially requires. And ere we take a glance at Mr. Wallace's labours, let us consider how long they have occupied. This is shown in the following passage from the preface to the work. Mr. Wallace says:—"The detailed study of several groups of the birds and insects collected by myself in the East brought prominently before me some of the curious problems of geographical distribution; but I

* "The Geographical Distribution of Animals; with a Study of the Relations of Living and Extinct Faunas, as elucidating the Past Changes of the Earth's Surface." By Alfred William Wallace. With Maps and Illustrations. 2 vols. London: Macmillan & Co. 1876.

should hardly have ventured to treat the whole subject had it not been for the kind encouragement of Mr. Darwin and Professor Newton, who, about six years ago, both suggested that I should undertake the task. I accordingly set to work, but soon became discouraged by the great dearth of materials in many groups, the absence of general systematic works, and the excessive confusion that pervaded the classification. Neither was it easy to decide on any satisfactory method of treating the subject. During the next two years, however, several important catalogues and systematic treatises appeared, which induced me to resume my work, and during the last three years it has occupied a large portion of my time."

And what, the general reader will probably ask, has the author of the work attempted? We shall endeavour to explain. Animal life exists in almost every portion of the globe; indeed, for convenience, we may assume that on every portion of the earth animal life is apparent. But we find as we travel through, suppose the forests of Brazil, or the regions of Upper India, or in the Arctic provinces, or again in Australia, a very different set of animals. Thus in one we find all the creatures have, more or less, marsupial pouches, as the kangaroo; in another we find the old-world apes; in a third we find elephants, and so on. Then again in point of time—that is, suppose a million of years—we find similarly a peculiarity of distribution of animal existence. For example, we find one type of animals succeeding another as we pass from the older to the more recent fossiliferous deposits. Now in both these cases it is of importance to find out how it happens that such different forms of life came in these localities, both of space or of surface, and of depth. Of course, if you took the view that every animal was separately created, there would be at once an end to the whole discussion; for then you would have taken it for granted, not that the animal of any particular locality arrived there ages ago from elsewhere, but that it was created on the spot. That would be an exceedingly singular view; and, unfortunately for his convenience, the scientific man must give it up at once. Then for him comes the question, How did these several races extend from one part of the world to another? Why, for example, should we find fossil in this country animals the same as those now living in Australia?

It is to this excessively difficult task that Mr. Wallace has partly devoted his attention, though of course he has had in the first instance to endeavour to arrange all the animals which are on the globe into a series of groups, so as to have those that are closely related to each other as to distribution as much together as possible. And Mr. Wallace has discussed the various schemes that have been suggested by different writers, and he has come to the conclusion that there are—as long since proposed by Mr. Sclater—six regions into which the world may be divided. These are (1) *Palæartic,* which includes North Europe, Mediterranean, Siberia, and Japan. (2) *Ethiopian,* which comprises East, West, and South Africa and Madagascar. (3) *Oriental,* which includes Hindostan, Ceylon, Indo-China, and Indo-Malaya. (4) *Australian,* including Austra-Malaya, Australia, Polynesia, and New Zealand. (5) *Neotropical,* including Chili, Brazil, Mexico, and Antilles; and (6) *Nearctic,* comprehending California, the Rocky Mountains, the Alleghanies, and Canada. Now taking these great groups as the primary division of the animal world, the author traces every genus of vertebrate animals along these lines—mammals, birds, reptiles, and fish. And with this labour is the greater part of these two huge volumes filled. The general reader will find a series of engravings arranged as plates throughout the volume, which will much help toward putting the ideas of the author clearly before him. Each of these represent a division, and shows together the animals that are included in it. Mr. Wallace thinks them well executed; but we do not at all agree with him, for we consider them hard, and generally badly drawn. Still, they lend a great interest to the work. The maps, which are abundant, are also very valuable; but we think the author's method of indicating the different degrees of height by differences of shading is extremely perplexing, for the distinctions are not absolute but gradual, and the confusion resulting from this is very great.

But even admitting these slight objections, the treatise is one of the highest scientific importance; and for ourselves we must express our extreme gratitude to the author for his labours—labours executed purely for the benefit of science, and which can never be repaid by any monetary return they may bring.

WHAT IS CHOLERA?*

ON this important subject we have now before us two works, which, though differing in character—the one being an elaborate Report of the progress of a single epidemic in America, which extends over more than 1,000 pages; the other an equally important treatise on Asiatic cholera as it presents itself to our notice in India—have yet much that is in common between them. We shall take the American volume first. In this most valuable Report on the progress of a single epidemic, it is proved conclusively that the disease did not arise spontaneously, but was introduced by immigrants from European countries. Of course there is a certain amount of evidence in this work that tends to an opposite conclusion, but it is almost exclusively negative in character; and where any opinion adverse to the belief in the spread of the disease from Europe, and primarily from India, is expressed, it is generally the idea of some individual reporter who has had little or no experience in the subject.

Throughout the entire work, which includes the various smaller reports of hundreds of physicians, we notice the same features in the history of the disease that are presented by every undoubted case of Asiatic cholera. And we are glad to observe that the author, or rather the editor of the work, points to the extreme importance of attending to the very first symptoms of diarrhœa that present themselves. It is really because of the neglect of this matter that cholera produces such an intense fatality. We observe that these American cases are precisely similar to those which we have seen in this country, and those which were unfortunately amply abundant in the East

* "The Cholera Epidemic of 1873 in the United States." By John M. Woodworth, M.D. Washington: Government Printing Office. 1875.—"A History of Asiatic Cholera." By C. Macnamara, F.C.U., Surgeon to the Westminster Hospital. London: Macmillan & Co. 1876.

Indies. That is to say, first there is diarrhœa, which is painless but copious; then pain, vomiting, cramps of the extremities, sinking of temperature, blueness of skin, collapse and death. It is to be noticed that these Reports include those both of the civil and military services, and that the latter is infinitely the most lucid, full and scientific. There is also in this volume a valuable notice of previous epidemics, from the time of Hippocrates (B.C. 460-370) down to the year 1873. We note also an important series of maps and plans, by which the reader is greatly assisted in arriving at his conclusions. But perhaps the most valuable part of this American Report is the portion devoted to bibliography. This has been carefully edited by Mr. J. S. Billings, and extending as it does over more than 300 pages, includes, in alphabetical order, everything that has been written on the subject since it began to have any literature connected with it. This one feature will make it a treatise that must be referred to by all writers and readers on the subject of cholera. Indeed this Report is a most thoroughly creditable one.

Of Dr. Macnamara's work—which is in some respects quite a different one, having to do rather with the origin both as to locality and nature of the disease—we have not the less highly to speak in praise. It is a work which the author has addressed to the educated lay reader as well as to the physician, but we fear the number of persons outside the medical world who are interested in cholera is extremely small. However, the book is of importance to all who consider the question, How is this plague to be arrested? And we are glad to see that Mr. Macnamara holds distinctly to the theory that cholera is only communicable through the swallowing of some portion of the discharge which has come from a patient suffering with the disease. Thus he believes—as we and nearly all rational physicians do—that the disease is not by any means infectious, and that it is not contagious in the ordinary sense of the word. He believes, then, that it is conveyed from town to town, and from country to country—in point of fact, from Bombay to New York—by the medium of water. Of course ships may not have their water infected—though there are undoubted cases cited from the Crimean war, in which the water was the means of convoy—but then the disease has not had time during the voyage to die out, and thus cases are brought to the different intervening ports—let us say, between Bombay and London. And the author deals exhaustively with the many theories that have been, from time to time, put forward. To account for the propagation of the malady thus, he deals very fully with the influence of winds in spreading the disease; and he shows, in the most conclusive way, that the atmosphere has no agency whatever in the distribution of cholera. In a similar manner he treats the subject of food and meteorological influences, as heat, fog, &c., and he shows that they have no influence over either the origin or the spread of this fearful affection.

We think that Mr. Macnamara has, on the whole, gone too fully into the several courses that cholera has taken from time to time, for we are of opinion that a very brief abstract would have been amply sufficient. We note also that he has said almost nothing as to the primary source of the fungus—for fungus it undoubtedly is which originates the malady—and we think that Dr. Lewis has already done some good work in this direction. Still the book is a most valuable one, and must be carefully read by all who are interested in the questions, How does cholera come in the first instance? and how is it passed from human being to human being in the second?

THE BOOK OF WEATHER SCIENCE.*

We doubt whether Messrs. King could have chosen a better subject for a scientific book, and we are sure that, having made the selection, they could not have hit upon one who is not only one of our best meteorologists, but who is in addition a clear and concise writer, more successsully than they have done in the case of this volume, called "Weather Charts and Storm Warnings." Mr. Scott has here given us an insight into the working of the Meteorological Office, and the production of those maps which everyone now sees in the daily papers, but which few, we fear, understand. However, it will be discreditable now not to comprehend clearly the tracings that are every day published in the press. The author has had a double difficulty to deal with in the production of the present volume, for he has had to bear the brunt of issuing the first work that has ever appeared on the subject; and, in addition, he has had to prepare a series of statements on some points that are, as he is obliged to confess, as yet hardly satisfactorily understood. Still, he has laid the question on which he has written clearly and without prejudice before the reader, while at the same time he writes as a true scientific worker, who is prepared to see many of the theories now adopted completely annihilated in course of time. He says that he aims at discussing the present condition of "weather knowledge, as distinguished from the science of meteorology itself. . . . In treating of a science now in process of rapid development, it can only be expected that every year will add to our knowledge, and that many of the principles stated in these pages will be extended or modified by the results of subsequent experience."

The book is divided into eight chapters, which cover about 150 pages. These deal with the following questions:—The materials available for weather study; the wind; the barometer; gradients; cyclones and anticyclones; the motion of storms and the agencies which appear to affect it; the use of weather charts; and, lastly, storm-warnings. Then follow a series of appendices, A, B, C, and D, which refer to certain remarkable weather reports taken by automatic instruments in Valencia, Aberdeen, and Falmouth. Of course the author has not much to say on the first subject in the above list. The materials are almost as well known as the weather-glass itself. Still, some of Mr. Scott's remarks must strike the general reader as of importance. For example, in telling us that the chance of rain depends to a great extent on the degree of humidity of the air, and that "if

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* "Weather Charts and Storm Warnings." By Robert H. Scott, M.A., F.R.S. With numerous illustrations. London: Henry S. King & Co. 1876.

we are dealing with reports from an extensive tract of country, as North America, or the continent of Europe, the distribution of the moisture or of the vapour-tension will afford great assistance in tracing out the probable motion of storms." He here shows us, what many are not aware of, the great importance of the wet-bulb thermometer, for it is by means of this instrument that the amount of moisture in the atmosphere is ascertained. And further on he points out the important fact that most of our meteorological stations are situated at the sea-side, where the atmosphere is almost always laden with moisture. They are, therefore, by no means so valuable for the determination of advancing rain as the more inland observatories.

In regard to the barometer, or rather the weather-glass, taken by itself as a guide to the state of the weather, Mr. Scott points out that this method is utterly absurd. We must quote him again on this point:—"Let us take, for example, the word 'Change.' This is placed opposite the reading '29·5 inches,' which reading is naturally supposed to be taken at sea-level. If the barometer be removed to a station situated say 500 feet above that level, the corresponding reading will be about 29·0 inches, so that the whole scale will be half an inch out, and the error will be greater the more considerable the height of the station. The lettering is, therefore, again wrong, because it does not take account of the necessary reduction of the reading to sea-level. Once more, the range of the barometer is far greater in winter than in summer, so that the reading which corresponds to 'Fair' should be much nearer to 'Change' in summer than in winter . . . *The words are, in fact, little less than utter nonsense.*"

The subject of gradients is very fully gone into by Mr. Scott, and the reader will follow his remarks with considerable interest. The chapter on cyclones and anti-cyclones shows us clearly how much work remains to be done to satisfactorily clear up our knowledge on the subjects. Indeed, on this question, which is complex enough, we do not see that very many conclusions can be drawn. However, such as they are, the author gives them to us. Another problem, that is certain to be more clearly ascertained as researches go on, is that of the motion of storms and the agencies affecting it. On this, too, Mr. Scott has told us all that is known. Of much interest are the author's observations on the connection (for there is an undoubted connection) between sun-spots and cyclones. He says, "Of late years Mr. Meldrum, of the Mauritius, has shown that the cyclones, for which that district of the Indian Ocean enjoys an unenviable notoriety, have been more frequent in some years than in others, and that these epochs of maximum frequency occur at intervals of about eleven years, coinciding with those of maximum sun-spot frequency." He then asks the question, why it has been left to Mr. Meldrum to arrive at this discovery? and he gives two excellent reasons why it should have been so.

Then he points out the nature of the well-known storm-signals—which were frequent enough in Admiral Fitzroy's time—and he shows us what we certainly are surprised to learn, that there has been great success in the prophetic department of the Meteorological Office in regard to its signals to Hamburgh. He says, "A system of warnings for Hamburgh, from our office, has been in operation since 1867, and the general results are that

301 warning messages were issued from London in the course of seven years. Seventy-two per cent. of these warnings were followed by gales, while in only three cases did the storm outrun the message." Although this result is manifestly better than any that has been attained with regard to storms affecting this country, still we may hope that further experience will tend to render our own storm-signals more productive of good than they are at present; although Mr. Scott says, on p. 144, that "we are able to maintain a general success of nearly eighty per cent. for our warnings"—a circumstance which will justify those who maintained that the Meteorological Office was wrong when at first they discontinued Admiral Fitzroy's system.

And with this subject we have come to the end of "Weather Charts and Storm Warnings," which we must pronounce, in conclusion, to be one of the most interesting and instructive volumes that has reached our hands for a very long time.

THE MOON.*

THIS book was undertaken with the view of promoting the study of Selenography. Unfortunately it is so large and costly a work as to be calculated to act as a deterrent from that rather dry, because heretofore unproductive pursuit. It was very well for Mr. Nasmyth to produce a costly book on the moon, because his work was intended for all who take interest in the wonders of astronomy; and, adorned as it was with many very elaborate and beautiful photographs, could not fail to be generally attractive. But a work like the present can only meet the wants of a very limited section of the astronomical public, and probably only a small portion of those even who take special interest in selenography would care to purchase a book whose retail price is a guinea and a half. We fear, therefore, that Mr. Neison may have to wait long before the second edition referred to in the preface of this work is called for, unless indeed he has prudently restricted the first edition to a few hundreds instead of the customary thousand, or the larger numbers to which publishers extend the first editions of more attractive works.

Regarding this work as a general treatise, to which character it in some degree aspires, we must confess we cannot perceive its *raison d'être*. It gives the history of lunar research too sketchily to be of much value in that respect, and only touches upon the peculiarities of the lunar motions, without a full account of which a general treatise on the moon is like Hamlet without the Prince of Denmark. Mr. Neison enters into a well-meant but perfectly futile attempt to show that those astronomers have been altogether mistaken who have asserted that the moon "has no atmosphere of any appreciable importance," and generally that the moon is at present an utterly unfit abode for any kind of life, animal or vegetable. And here we cannot but notice a very objectionable feature of the work—the cavalier, we

* "The Moon: and the Condition and Configurations of its Surface." By Edmund Neison, F.R.A.S. London: Longmans. 1876.

may almost say insolent tone, in which our author sets on one side the opinions of men like Sir J. Herschel, Mädler, and others, in some cases without even saying whose opinion it is which he thus rejects. One sentence of the preface is so characteristic in this respect, that we quote it in full :—"As it has been in general assumed, entirely without any foundation, that the moon can have no atmosphere of any appreciable importance, it has been considered desirable to point out how entirely baseless this view is, and to show not only that the moon may possess an atmosphere relatively little inferior to the earth's, but also that the entire evidence we possess on this subject is strongly favourable to the moon possessing such an atmosphere." We need hardly say that Mr. Neison entirely fails to carry out this vaunt, or that the great astronomers whose opinions he so cavalierly sets on one side have not adopted baseless views or made assumptions "entirely without any foundation." We believe Mr. Neison is a comparatively young man, and he has manifestly much to learn about the *convenances* of scientific writing. Will he pardon us if we point out that whereas it is perfectly legitimate and proper for any man to advocate views opposed to those held by the highest authorities, it is altogether improper to assert of the opinions of such men that they are "utterly baseless," "entirely without foundation," and so forth. Even the wisest, as we all know, may err, but only the most foolish would adopt opinions without a particle of evidence in their favour, and the great men whom Mr. Neison professes to controvert were not foolish by any means.

To indicate the character of Mr. Neison's reasoning on these matters it will only be necessary to remark that after expressing the opinion that the surface density of the moon's atmosphere may be equal to about 1-300th part of the density of our earth's atmosphere, he maintains that this exceedingly rare air would decrease considerably the heat of the lunar day and the cold of the lunar night. It is true he believes that immense quantities of aqueous vapour rise into the upper strata of the lunar atmosphere, interrupting the solar heat, and "preventing the solid body of the moon ever rising above its mean value" (whatever that may mean), while "in the same way the fall of the lunar temperature during the long lunar night would be prevented by a similar cause"—a most astounding assertion in the presence of the established laws of physics and of what is known about the moon's condition.

The selenographical portion of the work may be divided into two sections, first, the maps and descriptive matter; secondly, the formulæ. The former section is on the whole good, but would have been much improved if the maps had been more uniform in character; as it is, some details are introduced which would require a good telescope to show them, and some omitted which a very moderate telescopic power would reveal. The formulæ are for the most part useless to selenographers. Those who can follow them as they appear—of whom we may be permitted to doubt whether Mr. Neison himself is one—could have no occasion for them in this book; but those not sufficiently acquainted with mathematics to obtain all the formulæ necessary for selenographical work, would most assuredly not be benefited by this section of the work, which does not possess a single explanatory plate or woodcut, though it extends over nearly fifty pages.

THE WARFARE OF SCIENCE.*

PEOPLE who have received sufficient education to tell them of the history of Europe during the middle or dark ages, are aware of the fearful contests which took place between religion—then under the guardianship of Papal administration—and science, the pursuit then of the few. The history of Galileo, Vesalius, Copernicus—or Kopernik, as he is sometimes styled—which ought to be popularised most extensively, shows what the Church has done towards repressing knowledge in times gone by. And in this respect we must not be too severe on the Roman Church, though it was powerfully excited against the progress of science; for we find that even Luther himself, whom Protestants are always singing the praises of, was in his way as completely opposed to scientific advance as many of the Papal inquisitors. Dr. Dickson shows us this in the following passage :—"Justice compels me to say that the founders of Protestantism were no less zealous against the new scientific doctrine. Said Martin Luther, 'People gave ear to an upstart astrologer who strove to show that the earth revolves, not the heavens or firmament, the sun and the moon. Whoever wishes to appear clever must devise some new system, which of all systems is, of course, the very best. This fool wishes to reverse the entire science of astronomy. But Sacred Scripture tells us that Joshua commanded the sun to stand still and not the earth.'" And to this quotation from Luther might be added many others, more especially from the writings of Philip Melancthon, all of which condemn, not less strongly than the Papal powers, the scientific philosopher.

But it is remarkable that the power of the Church—whether that of Rome or of England—is exerted as industriously, if not as successfully, against science in the present day as it was in the times of Pope Paul V. There is no incorrectness in the following statement which is made by Dr. Tyndall in the preface to the book before us :—"In our day the Roman Church above all others aims at the revival and perpetuation of this wrong—striving after a domination which she never fitted herself to exercise, and which if exercised could only bring calamity on the human race. Ignorance alone can give her any chance of success; but with ignorance to work upon, her conduct in Spain may be taken as a gentle illustration of what it would do elsewhere. Gentle, I say, because unabashed as she seems, we must ascribe some power of restraint to the knowledge that her operations are carried on in the full blaze of intellectual day."

But if we would see how far religious opposition to freedom of thought has gone even within the past twenty-five years, we have only to refer to two instances which Dr. White very wisely calls attention to. In 1864 a number of men in England drew up a declaration which was signed by a definite few, who expressed their "sincere regret that researches into scientific thought are perverted by some in our time *into occasion for casting doubt upon the truth and authority of the Holy Scriptures.*" Nine-tenths of

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* "The Warfare of Science." By A. D. White, LL.D., President of Cornell University, with Prefatory Note by Professor Tyndall. London: Henry S. King and Co. 1876.

the leading scientific men in the country refused their signatures, whilst Sir J. Herschel, Sir J. Bowring, and Sir W. Hamilton administered through the press the most crushing rebuke to those who had got up the circular. And finally Professor De Morgan, in a parody, "covered memorial and memorialists with ridicule." We trust that this little book will help to allay some of the fears which religious people feel in regard to science, which can never come into conflict with genuine religion. The one is a mass of teaching all deduced from facts. The other has to do with those peculiar yearnings which all possess at some time or other, and which cannot be suppressed by any amount of teaching in minds of a certain stamp.

ELEMENTARY BOTANY.*

THIS is, with one or two exceptions, an admirable little work, well illustrated and cleverly written, while its woodcuts are most numerous—a point of immense advantage—and are generally well done. Withal, when we state that it is published at sixpence—which means that it is sold at about fourpence—our readers will be not a little astonished. The writer, Mr. W. Bland, is master of the Educational School at Duffield, and he has certainly done well in getting up such a book for his own and other pupils. The work is well done, save in the horrible selection of that barbarism of barbarisms, an artificial system of classification. Why did not Mr. Bland choose the excellent natural system to be found in Bentham's "Handbook of the British Flora"? We think, too, the postscript by the Rev. J. Smith might have been written in plain English, or omitted altogether. We object to sentences for children such as "Corrosive sublimate *plus* saturation," and that singularly unnecessary and pedantic expression, "culinary heat."

A POPULAR FERN BOOK.†

WE think there is an abundance of books in the market on ferns; indeed, we may almost say that it is glutted. And therefore, unless a book is written on this subject which has peculiar views of its own which tend to make it distinct from those which have gone before it, we think there is no justification for its publication. Well, now, can we say aught of the work before us that will help us to place it in a category of its own? We think we can. It is a book written by one who is evidently an intense lover of fern-life, and it is intended rather to awake a love for fern culture than to help the amateur botanist to study the group as a whole, or to find out any particular species he may have come across in his wanderings in the

* "Notes of Lessons on Elementary Botany." By W. Bland. Part I., First Year's Course, with 140 illustrations. London: Bemrose and Sons.

† "The Fern Paradise. A Plea for the Culture of Ferns." By F. G. Heath. Second edition. London: Hodder & Stoughton. 1876.

country. Its *raison d'être* is therefore distinct enough. How has the author done his work? On the whole, well. Still there are objections to be raised. In the first place we accuse him of a desire to make a book. This is quite clear. Indeed, everything that has been said in the nearly 300 pages might have been easily put into half that number. Then, again, the author is lax and talkative where he should be precise and terse. We note also an utter absence of illustrations, save the coloured frontispiece. This is a mistake, especially as the author admits that his descriptions are inexact. How can the reader use the book? Still it has passed through a second edition, and we suppose that is an adequate reply to our queries. Nevertheless we fancy if illustrations were employed it would soon reach a third edition.

VICTORIAN GEOLOGY AND PALÆONTOLOGY.*

THE catalogue by Mr. Ulrich contains a detailed description of an extensive series of rocks, illustrative of the geology of Victoria, the classification being mainly based on that given by Dr. Zirkel in his work on Petrography. Interspersed throughout are some valuable notes on the distribution and characters of the various rocks, and their modes of occurrence. From these it appears that the basalt rocks are extensively developed in Victoria, being estimated to occupy from 6,000 to 7,000 square miles of the surface. They appear to belong to two distinct periods; the older, and more widely distributed, occurred at the end of the miocene epoch, the newer eruption commencing towards the close of the pliocene epoch, and seems to have continued into the most recent post-pliocene times, the country occupied by the two basaltic flows differing very much in its physical features. Of the Palæozoic series, the Silurian rocks occupy a large area, and are estimated to be about 35,000 feet in thickness, and as regards the mineral capabilities, the Silurian, as a whole, is the most important rock formation to the gold miner, on account of its containing the matrix of the gold in the number of veins, lodes, and reefs of quartz that traverse it.

The Tertiary strata, however, whether regarded in their economical or physical aspects, occupy by far the most prominent place in Victorian geological history—either of sedimentary or volcanic origin. The strata of this period are distributed over fully one-half of the surface of the colony, varying in thickness from a few to more than 300 feet, and ranging from the sea-level to elevations of over 4,000 feet. They are of miocene, pliocene, and recent ages, the two latter having a far wider surface range than the older marine tertiary deposits, and are the most important, as they embrace the auriferous drifts which, with the associated streams of basalt-lava, belong at least to three distinct periods of deposition, the earliest not older than the pliocene, and the newest is probably due to causes still in operation.

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* "A Descriptive Catalogue of the Specimens in the Industrial and Technological Museum, illustrating the Rock System of Victoria." By G. H. F. Ulrich, M.E., F.G.S. Melbourne: 1875.—"Prodomus of the Palæontology of Victoria." By Prof. F. McCoy. Decade III. Melbourne: 1876.

The decade by Professor McCoy is a continuation of the description of the characteristic Victorian organic remains, and contains a number of illustrations of the fossils of the tertiary formations, and some other forms interesting to the geologist. Among these is much additional information as to the characters and dentition of the singular animal, the marsupial lion of Owen, and illustrations of some new tertiary species of *Cypræa*, *Aturia*, *Pleurotomaria*, and *Trigonia*, the latter two genera abounding in the mesozoic rocks, but of excessive rarity in recent and tertiary times, so that the species here described form an interesting addition to the history of the distribution of these genera in time and space. Some species of trilobites are also figured which are absolutely identical with forms abounding in the upper Silurian rocks of Europe—one of them British, and the other common in the Silurian basin of Bohemia—showing the wide range of similar species of trilobites, like those of graptolites described by Prof. McCoy in a previous decade in the seas of the palæozoic period.

THE SHELL MOUNDS OF FLORIDA.*

THIS memoir by the late Professor J. Wyman relates almost exclusively to the shell mounds and shell fields on the banks of the St. John's River in East Florida, which, as far as at present known, were the dwelling-places of the earliest inhabitants of the region through which this river flows. But little is known of the origin of these antiquities, which were long considered to be of natural and not of artificial origin. These mounds consist entirely of certain species of *Unio*, *Ampullaria*, and *Paludina*, of which the latter forms the largest portion of every mound, and with a few Unios the whole of some. Sometimes one species forms considerable deposits by themselves without the admixture of the others, as if at certain times they had been exclusively used as food; occasionally other shells, as Melaniæ and Helices are found, but in very small numbers. The mounds of St. John's appear to differ from the shell mounds of other rivers of the United States, which latter consist almost exclusively of Unios, those of St. John's being peculiar as affording the only, or at least the chief, instances in which the Ampullarias and Paludinas have become to so large an extent articles of food. They are also different as to their characteristics from the mounds on the sea-coast, which are composed entirely of marine species of shells.

These mounds are almost in all cases built close along the banks of the river, usually in the form of long ridges parallel to the shore, rising sometimes to the height of twenty feet or more, and resting either on ridges of sand or river mud, or on land slightly raised. They are often placed at the union of the river with a lagoon or creek, or at the outlet of a lake, such places probably giving the natives ready access in canoes to large areas for

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* "Freshwater Shell Mounds of the St. John's River, Florida." By. J. Wyman, Peabody Academy of Science. Fourth Memoir. Salem, Mass. 1875.

hunting and fishing, as well as for bringing together the different species of shells of which the mounds are made up.

From the various interesting facts brought together in the paper, and especially from the presence of fire-places, ashes, calcined shells, charcoal, and implements, together with the bones of edible animals, and occasionally those of man, found at various depths from top to bottom, and the absence of everything which might have been made by the white man, it seems certain that these mounds were the accumulations by, and the dwelling-places of, the earliest Indian inhabitants during the successive stages of their formation; and that some of them, and perhaps all, were completed and had been abandoned before the white man landed on the shores of Florida, for the signs of their great age are to be found not only in the mounds being covered by dense forests, but in their partial destruction by the river, the growth of swamps, and the consolidation of the shells through the percolation of water charged with lime.

AMERICAN GEOLOGICAL SURVEYS.*

OF the eight papers contained in the third number of the "Bulletin," two are devoted to geology, three comprise entomological subjects, one on a contour map of the United States, and the last on the grammatical structure of the Nez Percés language. Dr. Hayden's paper, which occupies the largest portion, and full of interesting details, is simply intended to render the beautiful pictorial sections which accompany it more intelligible to the general reader. They represent the scenery along the valleys of the Lower Gallatin and Madison rivers, together with two fine illustrations of different portions of the Yellowstone valley. The geological structure of the district is described, and some important facts are stated respecting the channels of the rivers of the West, from which it appears they do not necessarily lie along any fissures, anticlinal and synclinal depressions, but seem to have, in the majority of cases, cut their way directly across the line of fracture, thus carving out deep gorges through the loftiest mountain ranges. The notes on the tertiary and cretaceous periods of Kansas, by Mr. Mudge, shows that the former rocks occupy about 9,000 square miles, and that the cretaceous cover more than half of the surface of the State. In the paper by Mr. St. John, a further account is given to that previously published by Dr. Hayden, of the region of the headwaters of the Canadian river, which for its geologic interest and scenic features is considered not to be excelled by any similar extent of country in the West; from which it appears that the upper basin of the Canadian is underlaid by cretaceous strata, overlying which are deposits of tertiary age, including portions of the

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* "Notes of some Geological Sections of the Country about the Headwaters of the Missouri and Yellowstone Rivers." By F. V. Hayden, &c.— "Notes on the Geology of North-Eastern New Mexico." By O. St. John.— "Bulletin of the Geological and Geographical Survey of the Territories." Vol. II., Nos. 3, 4. Washington: 1876.

great lignitic formation, with its deposits of coal and iron. There are also evidences of considerable igneous action of recent date, as dykes are found traversing both the cretaceous and tertiary strata, altering the rocks through which they pass. Among the peculiarities of this basin of the Canadian is the terrace-like steps or benches—variable in height and distinctness of definition, according to the nature of the deposits—and which are the records of early shore-lines in the drainage of the ancient waters which occupied the region.

Other portions of the district are also described, of which many interesting details of the geological and physical structure are given. The remaining papers of No. 3, which complete the second volume of the "Bulletin," comprise descriptions of some carboniferous and cretaceous fossils from Vancouver's and Sucia Islands and other North-western localities; a note on a singular cretaceous crinoid, apparently related to Marsupites, from the Neobrara group of the Upper Missouri; and remarks on the geographical variation, especially as to size, among North American mammals.

SCIENTIFIC SUMMARY.

ASTRONOMY.

A Hitherto Unnoticed Inequality in the Moon's Motion in Longitude. —Professor S. Newcomb, of the Washington Observatory, has noticed an irregularity of the moon's motion which at present has received no explanation. After applying all the known corrections to the observations of the moon at Greenwich and Washington, from 1862 to 1874, "I was surprised," he writes, "to find systematic errors outstanding which could not be corrected in the lunar elements. Of their reality there could be no serious doubt, because the Greenwich and Washington observations agreed in showing them. At first I was disposed to attribute them to inequalities in the surface of the moon, but a more careful observation showed that they were periodic, and developed on the moon's longitude, being at first positive when the longitude was between 180° and 360°, and negative in the first semi-circumference. But this was found to be subject to a sensible alteration, the point of maximum positive error moving forward to about 0°, and that of negative error to 180° in the course of a few years' observations." The maximum amount of this inequality is $1''\cdot5$, and its period is 27·4304 days $\pm$ 0·0040 d. "There is a large preponderance of probabilities," says Professor Newcomb, "against the real period being either less than 27·42 days, or more than 27·44 days. No known term in the moon's longitude falls between these limits. The moon's sidereal period is 27·32 days, and her anomalistic period is 27·55 days, so that the new term falls half way between these two. The non-accordance of this period with that of any term heretofore sought for is the reason why this term has not before been noticed. A term of unknown period would not be remarked unless its magnitude was such as to visibly affect the individual comparisons of theory with observations, and Hansen's Tables, as corrected, are the first ones of which the residual errors are so small that a term of $1''\cdot5$ would be remarked in the comparison with observations."

The Atmosphere of Venus. — In 1849 Mädler and Clausen published some observations of Venus when near inferior conjunction, which showed that the sun's rays underwent a considerable refraction from the atmosphere of the planet. Mädler used the following formula:—

Let V= the angular distance between the centres of the sun and Venus; x, the prolongation of the cusps of the planet; S, the semi-diameter of the sun; ϕ, the radius vector of Venus. Then the horizontal refraction ζ_0 of the atmosphere of Venus was found from the equation,

$$\zeta_0 = \tfrac{1}{2}\left(\psi - \frac{S}{\phi}\right), \text{ where } \sin\psi = \sin V, \sin x.$$

By this formula Mädler found the horizontal refraction of the atmosphere of Venus to be 43′·7.

Strangely enough, the Astronomer Royal to whom Mädler sent his result, and who communicated an account of it to the Astronomical Society, detected no flaw in the formula, nor did the editor of the "Monthly Notices," who published it. Prof. Lyman, also, of Yale College, Conn., U.S., employed the formula unchanged to reduce his measures of the prolongation of the cusps, as observed with a 9-in. refractor, in 1866 and 1874, getting 45′·3 and 44′·5 for the horizontal refraction of the planet's atmosphere. We may learn from this how necessary it is to examine carefully every formula one may have occasion to employ in scientific work, no matter what authorities may have given it their sanction. For, after all, the formula was wrong. Mr. Neison, in a Paper recently read before the Astronomical Society, says, "A note by Mr. R. A. Proctor, in the 'Astronomical Register' (October 1875), induced me, as soon as I had the leisure, to examine this formula of Mädler's, when it was at once apparent that as it had been employed by Mädler and Lyman it was defective. Instead of the angle V, or the angular distance between the centres of the sun and Venus, as seen from the earth, which they had used, they should have employed the supplement of the angle between the sun and earth, as seen from Venus. In consequence of this error, the value which they have deduced from their observations for the horizontal refraction of the atmosphere of Venus is incorrect." Making the correction indicated by Mr. Proctor, Mr. Neison obtains as the mean result of Mädler's observations 54′·43. The four observations by Lyman in 1874 give 56′·34, 51′·54, 54′·63, and 51′·49, the mean value, 53′·50 agreeing closely with the result obtained from Mädler's. Combining both series with Lyman's results in 1866, Mr. Neison obtains 54′·65 as the probable horizontal refraction of the atmosphere of Venus; whence we may infer that the surface density of the atmosphere of Venus is not far on either side of 1·892 times that of the earth's.

Imagined Specular Reflexion from Surface of Venus.—Mr. Brett, the landscape painter, who some time since startled the astronomical world by stating that he had seen the solar corona when there was no eclipse and without telescopic aid, has lately made another remarkable discovery, stating in a paper read before the Astronomical Society (but cruelly suppressed by the editor), that he had detected unmistakable specular reflexion from the surface of Venus. Capt. Noble was led to investigate the question. "His first step was to examine the planet with the telescope in the usual way, and no signs of specular reflexion could be seen. He then, using a power of 255, interposed a graduated shade of dark glass. The first part to fade away was, of course, the fainter portion near the termination, then the cusps, and finally the bright part of the limb; not the slightest sign, trace, or indication of specular reflexion being visible."

Photometric Experiments upon Venus.—Mr. J. J. Plummer has made some photometric experiments upon Venus by a new and ingenious method, based on the fact that the light of Venus is so strong that objects placed in it cast a well-defined shadow. (Mr. Plummer somewhat strangely says that Venus is frequently bright enough to cast a well-defined shadow!) "The plan I have adopted," he says, "has been to compare the light of the planet with that of a standard sperm-candle burning 120 grains of wax per hour, and to vary the distance of this until the shadow it casts upon a screen of white paper has an equal degree of intensity to that given by the planet. It being found impossible to get the two shadows on the same screen, separate screens were arranged for each, and brought as near to each other as possible. The arrangement was therefore a modification of Rumford's photometer. The objects of which the shadows were observed were two equal cylindrical steel wires of $\frac{1}{14}$-inch diameter, placed in a dark room, nine feet in front of their respective screens; but the judgment was further assisted by noting also the shadows of the wooden laths to which the wires were attached, and of which the thickness was about $\frac{1}{2}$ inch. These conditions were preserved throughout the whole of the observations. To protect the candle from wind, it being necessarily placed in the open air, it was fixed within a lantern, which was itself enclosed, except upon one side, in a rough wooden box, painted a dull black. These precautions are believed to have been sufficient for their purpose." The result of these observations is to assign to Venus at her greatest brilliancy almost exactly one-800th part of the light of the full moon. As Bond's observations assign to Jupiter at mean opposition one-6430th of the light of the full moon, and to Venus at her greatest brilliancy 4·864 times the brightness of Jupiter, it follows that Mr. Plummer's estimate of the brightness of Venus exceeds by about 65 per cent. the estimate deducible from Bond's observations (one-1322nd part of the light of the full moon). Mr. Plummer considers that "since the methods employed are entirely dissimilar, and since Bond's investigation has chiefly to do with the moon and Jupiter, both of which Bond observed at altitudes generally much greater than that at which the observation of Venus is possible, this discordance does not prove much." Perhaps not; but, in our judgment, it disproves a good deal.

Proper Motion of Bright Spots on Jupiter.—Mr. Brett has observed that some spots on Jupiter during the late opposition were affected by a considerable proper motion. As these observations required only a good eye for position, reliance can in all probability be placed upon them. Mr. Brett has not corrected his computation for the effects of the planet's retrograde motion; taking that into account, the spots observed would seem to have travelled at a rate of fully 150 miles per hour. This is not quite so great as the proper motion of a rift observed by Baxendell of Manchester, one end of which travelled at a rate of 190 miles per hour for a period of at least six weeks. The spots observed by Mr. Brett were judged by him to be 6,000 miles in diameter, and from the character of their shading and shadows he regarded them as globular in form—an opinion which will probably not be accepted unless stronger evidence can be advanced in its favour.

The Rings of Saturn.—Mr. Trouvelot, of the Harvard Observatory, Cambridge, Mass., has made some interesting observations upon the rings of Saturn. From these it appears that the rings present all those characteristics which we should expect from their now generally recognised constitution. In particular the dark ring, the phenomena of which have been thought by some to be inconsistent with the theory that the rings are made up of multitudes of small satellites, has presented appearances for which no other theory seems able to account. The inferior portion of the dark ring, where it crosses the disc of the planet, loses itself in the planet's light. This ring again is no longer transparent across its entire width, but is denser near its exterior part, in such sort that from about the middle of its width to its exterior edge it does not permit the edge of the planet's disc to be seen through it. Lastly, the matter composing the dark ring is aggregated here and there in small masses, which almost entirely prevent the light of the planet from reaching the observer.

Duplicity of the Solar Dark Line 1474.—Professor Young has made an important discovery by showing that this now celebrated line is double, the components being separated by about $\frac{1}{7}$th of a division of Angstrom's scale, or by about $\frac{1}{40}$th of the distance between the D lines. Only one of the components belongs to the spectrum of iron, and doubtless the corona line which had been satisfactorily shown to correspond in position with 1474, accords with the other component, not with the component belonging to iron. To what element the corona line really appertains has yet to be determined.

BOTANY AND VEGETABLE PHYSIOLOGY.

The Celtic Race of Pears.—An interesting paper to botanists who are lovers of the antique is given in a late number of the "Journal of Botany" by Dr. Masters. The part of most antiquarian interest is that which refers to the Celtic race, to which the Persian, French, and Devonshire forms belong, and is itself a quotation. It says:—"Dr. Phené visited Brittany, to trace practically any connection—if such could be found—between the legends which connect the 'Isle of Apples' of Arthurian repute with that locality, and those which connect it with Britain. King Arthur, it appears. is supposed to have been buried either in the Island of Avalon (Glastonbury), in England, or in that of Aiguillon in Armorica, the equivalent of Isle of Avalon being Isle of Apples. An island in Loch Awe, in Argyllshire, has a Celtic legend containing the principal features of Arthurian story, but in this case the word is 'berries' instead of apples. These particulars were fully given in a paper read on June 10, 1875, by Dr. Phené before the Royal Historical Society, in which he expressed a belief that the legend of the mystical Arthur was derived from the character of Arjuna given in the Indian poem, "Máhá Bárata." After closely examining the Island in Loch Awe, and Avalon in Somersetshire, he concluded his researches by a visit to Armorica, Brittany. He there observed a tree which helped him to the apples of Avalon and the berries of Loch Awe, for the apples on the tree were berries. The specimen he has submitted to us is the *Pirus cordata* of Desvaux, and it is interesting to note, in support of Dr. Phené's argument, that it has been found in Western France—perhaps in South-western

England, if the plant found by Mr. Briggs near Plymouth, and called by Dr. Boswell-Syme 'Pyrus communis, var. Briggsii,' be the same—and nowhere else in Europe. Both countries had their western shores occupied, anterior to the invasion of the Cymry, by a peculiar race of people having strong Oriental characteristics, and which people some authors describe as occupying the country as far north as Argyllshire—the evidences of such occupation having been laid before the British Association at Bristol in September 1875, in Dr. Phené's paper on that subject—while the same tree is found on Mount Elbruz in North-east Persia—a country not remote from that which formed the arena of Arjuna's exploits, and whence it would seem to have been imported to the west of Europe."

On Floral Æstivation.—The Rev. G. Henslow read a paper on the above subject before the Linnean Society on June 1. Mr. Henslow referred to his previous paper read before the Society, in which he regarded the opposite as the fundamental arrangement of phyllotaxy in Dicotyledons, and described the various modifications of imbricate æstivation. Starting from the ordinary pentastichous or quincuncial mode, in which two leaves of the cycle are external and two internal, whilst one is half outside and half in, special attention was called to the "half-imbricate" and "imbricate proper" methods, in both of which there are one external, one internal, and three intermediate leaves; the "imbricate proper" is converted into the convolute mode, in which all the leaves are intermediate, by the first leaf of the cycle being overlapped by the adjacent third leaf. The "vexillary" and "cochlear" modes, and those of many other irregular flowers such as *Cassia,* are to be referred to the "half-imbricate." The author agreed with Professor A. Gray in distinguishing "convolute" from "contorted." A new theory of the nature of cruciferous flowers, which derived them from a primary type by symmetrical reduction of the parts in each whorl, was explained; and chorisis was objected to as an explanation of the pairs of long stamens. The frequency with which the corolla is found to develope subsequently to the stamens was also mentioned in objection to Pfeffer's view of the corolla of *Primula* being an outgrowth of the androecium.

The Hygroscopic Mechanism by which Seeds bury themselves was some time ago the subject of a paper read before the Linnean Society by Mr. Francis Darwin, the son of the celebrated naturalist, and it is thus reported by the "Journal of Botany" (No. 161):—"The seeds observed were those of several grasses and of *Anemone montana,* but *Stipa pennata* was specially examined. This has a strong awn, the lower part vertical and twisted with two knees, and a long horizontal upper feathered portion. Moisture causes the spiral portion to untwist and the horizontal part to revolve, the knees disappearing and the whole awn becoming straight; drought reverses the process. In nature the flat feathered portion is readily entangled in vegetation, and the seed rests vertically with its point on the soil. When the spiral untwists with moisture, the horizontal part being prevented from revolving, that motion is transferred to the seed, and to this being added pressure on its point it becomes screwed into the ground. With dryness and the reversal of the screw the seed is not drawn out again, but curiously is thrust deeper down by additional mechanism. Heat acts in the same way as moisture. The cause of torsion as explained by Hildebrandt and Hanstein the author thinks insufficient, and shows that the power resides in the individual cells of the awn, which when isolated behave precisely as the whole awn, with regard to moisture, heat, and dryness."

A Lichen rare in Great Britain.—The occurrence of *Thelocarpon Laureri* in Britain is so rare, that its appearance in great quantity is worth recording. Three habitats are given for it in Leighton's "Lichen-Flora," all in Shropshire; and, says Mr. W. Phillips, writing in the "Journal of Botany," No. 161, "I have now to add a fourth, also in the same county. In the autumn of 1874 a plantation on the Arcoll Hill, an outlier of the Wrekin, by some accident was set on fire, and a large portion was destroyed. The undergrowth, consisting of heather, bilberry, brakefern, &c., was so dry that no efforts were able to arrest the flames till the whole area enclosed by the cart-ways for drawing timber was left bare and black; these formed an effectual barrier and arrested the conflagration. The damage extended over several hundred acres. Last autumn a new growth of vegetation began to make its appearance on the charred surface, amongst which were conspicuous *Marchantia polymorpha*, *Funaria hygrometrica* (la Charbonnière), seedlings of *Pteris aquilina*, and a quantity of fungi, such as *Agaricus carbonarius*, Fr., *Peziza trachycarpa*, Curr., *Rhizina undulata*, Fr. On visiting the place this spring I found on the peaty portions a large quantity of *Thelocarpon Laureri*, in small patches from an inch to a foot across, extending over a very large area. At first sight I mistook it for the early growth of a Lichen-thallus, but when once recognised the eye became accustomed to its peculiar citron colour and scattered mode of growth."

CHEMISTRY.

Errors of the New Catalogue of the Loan Collection.—The "Academy" (August 12) has published a report on this book which has dealt justly, but severely, with the authors, whoever they are. It says:—"While the collection may be justly styled scientific, that term cannot be applied to the method in which the objects are classified in the catalogue, and many of the blunders are of such a character that it is hard to conceive it possible that they could escape the notice of a printer's reader, still less that they should pass unchallenged the scrutiny of a scientific editor." After giving a series of the errata, the article thus concludes:—"In printing the names and addresses of the contributors the same want of accuracy has been shown, and so we read of the 'Physical Institute of the University of Freiberg, Baden.' In many cases the foreign names of pieces of apparatus have not been translated at all, although an English equivalent is to be found without difficulty: we meet, for example, with such words as 'étuve,' 'stativ,' 'bobine,' &c.; others again which have been rendered into English are not in the form familiar to the man of science: such are, 'charcoal sticks' for carbon points, 'effective substance' for active principle, 'Grove pile,' 'chroïtes crystals,' 'rhomboïd of Iceland spar,' &c. The mode of rendering other scientific terms in common use in England is equally unhappy, and of these we may instance 'atterism,' 'apparatuses,' 'a chemical harmonica,' and

'Spee-gear, bottle in case,' whatever that may be; while the term 'pantigraph' is still retained in the introduction. The blunders appear to be impartially distributed over the various sub-sections; while we have 'wolfram phosphide,' 'siliconcalcium,' and 'molibdate of lead,' on the one hand, we meet with 'Kemala' and 'Saborandi' on the other; and among the rocks and minerals, 'Trooshte' for troostite, 'dimyte,' repeated, for dunite, 'Garnet fils,' 'Rosed felsyte,' 'Hokscharowite,' &c. Finally, we will confine ourselves to directing attention to about a dozen of the errors in the names of persons which we have met with during a superficial examination of this revised edition of the Catalogue:—'Andrew' for Andrews, 'Berzélius,' 'Bilstein' for Beilstein, 'Cloës' for Cloez, 'Sir Humphery Davy,' 'Eisenrohr' for Eisenlohr, 'Erltenmeyer' for Erlenmeyer, 'Fritzshe,' 'Kakuli' for Kekulé, 'Rutlerow,' 'Rudnen' for Rudneff, 'Schick' for Schiek, 'Siemgan' for Siemens, 'Smith' for Smithson, and 'Wrohlesky' for Wroblevsky."

Nitrogen and Albumen in the Milk of Women and Cows.—Dr. Leo Liebermann states in Liebig's Annalen (Bd. 181, Heft. 1), that both in Brunner's and in Hoppe-Seyler's method a considerable part of the albuminoids escapes precipitation. Haidler's method, on the other hand, gives the total amount of the lacteal albuminoids, which may also be entirely thrown down by means of tannin. In addition to casein and albumen a third and distinct albuminoid body is present, but there is no nitrogenous body found in milk except such as belong to the albuminoid class.

American Chemical Society.—The "Chemical News" of July 14 says that at a meeting of American chemists, held lately at the New York College of Pharmacy, it was resolved to form a society to be called "The American Chemical Society," and at a subsequent meeting the following officers and committees were appointed:—*President*—John W. Draper. *Vice-Presidents*—J. Lawrence Smith, Frederick A. Genth, E. Hilgard, J. W. Mallet, Charles F. Chandler, Henry Morton. *Corresponding Secretary*—George F. Barker. *Recording Secretary*—Isidor Walz. *Treasurer*—W. M. Habirshaw. *Librarian*—P. Casamajor. *Curators*—Edward Sherer, W. H. Nichols, Frederick Hoffmann. *Committees on Papers and Publications*—Albert R. Leeds, Herrmann Endemann, Elwyn Waller. *Committee on Nominations*—E. P. Eastwick, M. Alsberg, S. St. John, Chas. Fröbel, Chas. M. Stillwell.

The Action of certain Filters has been gone into by Mr. Alfred Wanklyn, who reports as follows in the "Chemical News" (July 14) on the action of the "Silicated Carbon Filters," which, as we have already reported in a former number of this journal, are unquestionably the best species of filter. A solution of hydrochlorate of morphia in common London water was prepared by taking 1·320 grms. of hydrochlorate of morphia, dissolving it in water, and diluting the solution to 10 litres. In this manner a solution containing 0·132 grm. of the hydrochlorate per litre of water was obtained. Submitted to the ammonia process, this solution was found to yield 2·60 m.grms. of albuminoid ammonia per litre. Five litres of this solution were then allowed to run through the same silicated carbon filter which had been employed for the experiments on quinine, described before, and the 5 litres of filtrate were then thrown away. In this manner the most simple displacement of the liquid occupying the pores of the filter was ensured.

About 5 more litres of the solution were next run through the filter, and the filtrate was examined with the following results:—Milligrams of albuminoid ammonia per litre of liquid—No. 1, 0·06; No. 2, 0·04. Showing how completely the filtration had removed the morphia from the solution. As a further corroboration, advantage was taken of the reducing properties possessed by morphia, which decolourised standard solution of permanganate, and which may be titrated with such a solution. Before submitting it to filtration, 100 cubic centimetres of the solution of morphia reduced 8·5 c.c. of decinormal permanganate solution. After filtration, 100 c.c. of the liquid did not reduce any appreciable quantity of the permanganate. Thus it has been proved that one single filtration through a thickness of 6 inches of "silicated carbon" is sufficient to remove morphia from a solution containing 132 m.grms. of the hydrochlorate of morphia in 1 litre of water (or 9·24 grains per gallon).

GEOLOGY AND PALÆONTOLOGY.

The Geology of the Carrara Marbles.—Mr. G. A. Lebour makes the following observations on this subject in the "Geological Magazine" (July). The statuary marbles of Carrara have in turn been referred to the—

1. Eruptive series. 1829, Savi.
2. Cretaceous. 1833, Savi.
3. Oolite (without further specialization). 1843, Savi.
4. Palæozoic, probably Carboniferous. 1845, Coquand.
5. Jurassic and Liassic. 1845, Pilla.
6. Infra-Lias and Rhætic. 1847, Pilla.
7. Lower Lias. 1851, Savi and Meneghini; 1856, Cocchi; 1862, Savi.
8. Base of Verrucano (Trias or Permian). 1862, Capellini.
9. Lower Carboniferous. 1864, Cocchi; 1875, Coquand. Generally admitted.

Let us hope that these ill-treated beds have now found a permanent rest. Still it is painful to see how long it has taken for the truth to prevail in this case. Had not the unlooked-for discovery of fossiliferous carboniferous beds taken place, the very clear stratigraphical evidence adduced by Coquand in 1845, strengthened by his determination of triassic beds at Spezia, would have gone for nought against the preconceived theories of high authority.

The Glaciers of the North Slope of the Alps.—At the meeting of the Geological Society (May 24), reported in the "Geological Magazine" (July), Professor Alphonse Favre, F.M.G.S., read a paper on the above subject. The author illustrated his remarks by a map on a scale of $\frac{1}{250000}$, showing the space occupied by the old Swiss glaciers at the time of their greatest extension, and founded in part upon evidence obtained since 1867, when he, in conjunction with Professor Studer and M. L. Soret, issued an "Appel aux Suisses" for the preservation of erratic blocks. He said that in existing glaciers two parts may be recognised—an upper one, the reservoir or feeding glacier, and a lower one, the flowing glacier. Applying this division to the old glaciers, it appears that in the glaciers of the Rhone and Rhine the flowing glacier which occupied the plain had a surface nearly equal to that of the feeding glacier which was situated in the mountains. By means of several tables M. Favre showed the height attained by these glaciers, their thickness, the slope of their upper surface, &c., at various points in the Alps, the Jura, and Swabia, and deduced as the result of the comparison of these numbers:—1. That the Rhone glacier passed over several of the chains of the Jura, and that the ice covering these, far from being an obstacle to the extension of the glaciers of the Alps, actually reinforced them, and served them as *relays*, the glaciers of the Jura having carried far on the Alpine erratic blocks. 2. That the slopes of the upper surface were variable, and were null, or nearly so, over considerable spaces.

On the Ice Age in Great Britain.—In a paper published in the "Proceedings of the Edinburgh Geological Society" Mr. Ralph Richardson gives the facts with regard to the shallow depths of ocean between Great Britain and Iceland and Greenland on one side and over the German Ocean on the other, and presents reasons for believing that there was dry land over the region in the glacial era; that the glaciers of Great Britain came over this emerged land from the north and west; and that the cold of the glacial era was due in part at least to the closing thus of the Arctic, and excluding thereby the Gulf Stream. The facts appear to sustain the conclusions. The depth between Britain and Iceland mostly does not exceed 100 fathoms, and nowhere exceeds 1,000; and one tract of sea extending in a straight line from the eastern coast of Greenland *viâ* Iceland and Faroe to Scotland does not exceed 500 fathoms. The depth of the sea in the English Channel is only about 20 fathoms, and the average depth of the North Sea or German Ocean is not over 40 fathoms, or 240 feet. The depth between Britain and Greenland is small compared with the average depth of the Atlantic. The author closes with the conclusion, that one of the oscillations of level, such as have often occurred over the earth's surface, had the effect to "unite Britain and Northern Europe with Greenland and the Arctic regions;" "to give the polar ice-fields access to Europe;" "to divert the course of the Gulf stream and free North-western Europe from its influence; and, in conjunction probably with some diminution in the influence of the sun, to produce a glacial epoch."

Ice and Ice-work in Newfoundland.—In a paper on this subject in the "Geological Magazine," Aug. 1876, Mr. J. Milne gives the following as the conclusion to be drawn from a series of observations on the subject:—"If Newfoundland has been steadily rising during past ages, as it now appears to have done at no very remote geological period, it may have been beneath the surface of the ocean. During the period when it was undergoing elevation, no doubt a considerable amount of débris and boulders were dropped by icebergs over its surface; when the Laurentian backbone, which would be the first land to emerge, reached the surface, it formed a barrier for the coast-ice which would carry its load of boulders and strew them with those of the bergs. This latter might to some degree have been influential in giving a definite character to the rising area. After the final emergence, the climate of Newfoundland might still have been a cold one, and the same highlands which gave birth to the coast-ice, probably next gave birth to glaciers which scooped and hollowed out a great portion of the remaining marine drift, and left the island with its present contour. After the raising of the great north-east and south-east ranges, first coast-ice flowed east and west, and afterwards the glaciers followed in a similar direction, and thus perhaps the origin of the boulders, those which are so curiously perched being due rather to the latter than to the former. Thus it would seem that icebergs and coast-ice preceded glaciers, but to say what might have come before the former of these agents would only be diving deeper into the depths of a sea of speculation."

The Ash-showers of Iceland.—Professor Nordenskiold, in the "Geological Magazine" (June), has an important paper on this interesting subject. From this we extract the following:—"Our knowledge of these eruptions, however, unfortunately is not as yet founded on any scientific examination; and it is perhaps the less necessary to repeat here the interesting accounts of those grand phenomena that have appeared in the newspapers, as I expect to have an opportunity another year of returning to the subject, since the region will probably be visited next summer by a distinguished geologist, well acquainted with the natural history of Iceland. I will only mention that the eruption began in the month of Dec. 1874, and then continued with shorter or longer intervals from numerous craters situated in the interior of the country, partly on Dyngjufjäll, partly in the northern part of Vatna-jökul, or in the region between these enormous glaciers and the great snow-clad volcano Herdabreid. The most plentiful ash-rain on Iceland itself took place in consequence of an eruption which began at the place last mentioned on March 29, and the ashes which fell in Scandinavia probably belong to the same point of time, in which case less than twenty-four hours was required for carrying the ashes from Iceland to Scandinavia; that is, for their passing over a distance of 200 Swedish miles, or 2,000 kilometres. Geological science has recorded many accounts of the fall of volcanic ashes, where the ashes have been carried by the wind to very remote regions; among others that ashes had already been carried, a couple of centuries ago, from Iceland to Bergen, on the west coast of Norway; but no example of so extensive a spreading of volcanic ashes with the wind, as from Iceland to the east coast of Sweden, is previously known. On Iceland the ashes fell in such quantity that at some places they covered the ground to a depth of 6 inches, and destroyed the pastures. The cloud of ashes was for several hours so close that the sunlight could not penetrate it, and lights required to be kindled in the middle of the day. The ashes must also have fallen in considerable quantity in the sea between Iceland and Norway, and on its bottom there are doubtless found places where the remains of such falls collect during centuries without any considerable mixture of foreign matter. Here must be formed thick beds of volcanic ashes, which in the course of geological ages gradually harden together, and are metamorphosed to rocks of nearly the same composition, and therefore also strongly resembling those which in molten form burst forth from the interior of the earth; and we have here, doubtless, the key to the extension over boundless regions of the earth of stratified so-called volcanic rocks, a circumstance to which I have already long ago drawn attention with reference to the occurrence of plutonic rocks regularly stratified in the polar regions."

American Earthquakes in the First Quarter of the Year.—In a good summary of recent earthquakes, which appears in the "New York Times" for July, Mr. C. G. Rockwood gives the following summary of the reported earthquakes during about the first three months of the present year (1876):—

Jan. 7.—Three shocks at the Island of St. Thomas, W.I., in the morning, the first at about 4 o'clock, the second at about 4.30, which was very severe, and the last three minutes later.

Jan. 7.—A shock at 2.20 P.M. at Warner and Contoocookville, N.H. Its apparent course was from west to east, and its duration two minutes.

Jan. 8.—A shock at 4.30 P.M. at Lockport, N.Y.

Jan. 15.—A severe shock at midnight at China, Me.

Jan. 21.—A shock between 3 and 4 A.M. at San José, Santa Cruz, and San Francisco, Cal.

Jan. 27.—Two shocks at Adrian, Mich.

Jan. 29.—A shock at 9.05 P.M. at Annapolis, Md.

Feb. 7.—A shock in the city of Mexico.

Feb. 27.—A shock at Detroit, Mich.

March 25.—Two slight shocks at 6 A.M. and 1 P.M. at Oaklands, Cal.

April 10.—A shock was felt in a large portion of St. Mary's County, Md., attended by a rumbling sound.

MEDICAL SCIENCE.

The Form in which Iron exists in the Spleen.—MM. Picard and Malassez, starting from the fact that the proportion of iron in the spleen is greater than can be accounted for by the blood it contains, proceed to enquire whether this iron is present in the form of some special compound, or simply in that of hæmoglobin fixed in the splenic tissue ("Comptes Rendus," April 10, 1876). The latter view was found to be the correct one. The following was the mode of investigation adopted. The gland was thoroughly washed out by allowing a stream of salt solution (shown by Kuhne to be incapable of dissolving hæmoglobin) to flow through it until it escaped colourless from the splenic vein. Notwithstanding the complete removal of the blood, the organ still retained its deep-red colour. A stream of distilled water was then admitted into the artery; it issued bright red from the vein. After about two litres had thus permeated the gland, the latter was found to have lost its red colour, and no longer to tinge the water flowing through it. The colouring matter dissolved by the distilled water was proved by its behaviour towards oxygen, carbonic oxide, and reducing agents, to be ordinary hæmoglobin. The decolourised spleen was then submitted to analysis, and found to be entirely free from iron. Hence the authors conclude that iron exists in the splenic tissue as a constituent of hæmoglobin identical with that of the red corpuscles of the blood.

A Poisonous Dye in Wines.—It has recently been discovered that the aniline dye known as fuchsine, or magenta, is largely employed by dishonest wine-growers in France for improving the colour of claret, and masking its dilution with water. Unpleasant symptoms have been observed to follow the use of this medicated beverage; and MM. Feltz and Ritter have accordingly made some experiments in order to ascertain how far the added colouring-matter ought to be blamed for them ("Comptes Rendus," Juin 26, 1876). They found that half a gramme of fuchsine in solution, taken on an empty stomach, caused deep redness of the ears, intense itching of the mouth, and slight swelling of the gums. The wine was stained of a deep red colour. When the dose was repeated day after day for a fortnight, diarrhœa and albuminuria were developed in addition to the above symptoms. When fuchsine was injected into the stomach or the veins of a dog, it produced effects similar to those observed in the human subject; when the dose was sufficiently large or frequently repeated, albumen invariably made its appearance in the urine; and this symptom was found to be due to a peculiar degeneration of the cortical substance of the kidneys. See also "Academy," Aug. 12.

Rescuing Drowning Persons.—M. Woilley is reported to have devised an instrument which he calls a *spirophore*, for resuscitating drowned persons and warding off the risk of death by asphyxia in certain diseases ("Comptes Rendus," Juin 19, 1876). It consists essentially of a metal cylinder, closed at its lower end, and large enough to contain the body of a full-grown man. The upper end of the cylinder is closed by an elastic india-rubber diaphragm, with a hole in the middle, through which the head of the patient projects. The interior of the cylinder is then partially exhausted by a sort of air-pump; with each stroke of the piston the chest of the patient expands, his diaphragm sinks, and air rushes into his respiratory passages. One advantage of this method of performing artificial respiration is that the air is never forced inte the lungs under a pressure higher than that of the atmosphere; there is no risk of damage being inflicted on the delicate pulmonary tissues, as sometimes happens when insufflation is resorted to. Experiments on the dead subject showed that the average amount of air introduced at each inspiration was nearly twice as great as that drawn in during ordinary breathing. The main objection to this ingenious contrivance lies on the surface—it is not likely to be at hand when wanted, and cannot therefore compete with methods of artificial respiration which, like those of Marshall Hall and Silvester, require nothing more than a certain degree of skill and readiness on the part of the bystanders.

METALLURGY, MINERALOGY, AND MINING.

Roscoelite a Vanadium Mineral.—In "Silliman's American Journal" (July) Mr. F. A. Genth says that he is indebted to Dr. James Blake, of San Francisco, California, for a small quantity of the very interesting mineral, which he called "Roscoelite," in honour of Professor Roscoe, whose important investigations have put vanadium in its proper place among the elements. Roscoelite occurs in small seams, varying in thickness from $\frac{1}{20}$ to $\frac{1}{10}$ of an inch in a decomposed yellowish, brownish, or greenish rock. These seams are made up of small micaceous scales, sometimes $\frac{1}{4}$ of an inch in length, mostly smaller and frequently arranged in stellate or fan-shaped groups. They show an eminent basal cleavage. Soft. The specific gravity of the purest scales (showing less than one per cent. of impurities) was found to be 2·938 ; another specimen of less purity gave 2·921. Lustre pearly, inclining to submetallic. Colour dark clove-brown to greenish-brown, sometimes dark brownish-green. Before the blow-pipe it fuses easily to a black glass, colouring the flame slightly pink. With salt of phosphorus gives a skeleton of silicic acid, a dark yellow bead in the oxidising flame, and an emerald-green bead in the reducing flame. Only slightly acted upon by acids, even by boiling concentrated sulphuric acid: but readily decomposed by dilute sulphuric acid, when heated in a sealed tube at a temperature of about 180° C., leaving the silicic acid in the form of white pearly scales, and yielding a deep bluish-green solution. With sodic carbonate it fuses to a white mass.

The Chemical Composition of Durangite.—Mr. G. Brush, whom mineralogical readers will remember described this mineral as long ago as 1869, says that he is again indebted to Mr. Henry G. Hanks, of San Francisco, for a new supply of the crystals obtained in recent explorations. These crystals are much smaller than those previously examined, being from one to three millimeters in diameter, and they are of a darker shade of colour. The former were loose detached crystals, while these are associated with, and in some cases attached to, rolled fragments of crystallised hematite and cassiterite. The density of the small dark-coloured crystals is 4·07, while that of the purest of the bright-coloured crystals before described is 3·937. In all other physical characters there is a perfect correspondence between the two varieties. The chemical examination of the dark-coloured small crystals has been undertaken, at my request, by my assistant, Mr. George W. Hawes, first to estimate the amount of fluorine in the mineral, which in two determinations he found to be 7·67 and 7·49 per cent., and Mr. Hawes as also placed at my disposal for this article a complete analysis of this variety of the mineral. The fluorine was determined directly by Wöhler's method as modified by Fresenius. To determine the arsenic acid and the bases, the mineral was decomposed by sulphuric acid, and the arsenic weighed as sulphide; the alumina, iron, and manganese obtained in the analysis were carefully examined to ascertain their purity. The soda and lithia were weighed as sulphates and then converted into chlorides and separated by ether and alcohol. The results of the analysis are as follows:—

	I.	II.
Arsenic acid	53·11	—
Alumina	17·19	—
Ferric oxide	9·23	—
Manganic oxide	2·08	—
Soda	13·06	—
Lithia	0·65	—
Fluorine	7·67	7·49
	102·99	

Silliman's American Journal, June 1876.

MICROSCOPY.

Mr. Sorby on Count Castracane's views on ultimate Vision with the Microscope.—The President of the Microscopical Society (Mr. W. C. Sorby) gave a valuable address this year, in which he referred to and elucidated Helmholtz's views. This address was subsequently read by Count Castracane, who made a series of observations, showing that vision extended further than Helmholtz's views would allow. These are now commented on as follows by Mr. Sorby:—"With reference to the facts here described by Count Castracane, I wish to offer a few remarks. It appears to me that the visibility of the fine lines of Nobert's test-plates depends on several different circumstances. The light must be thrown in such a manner as to be definitely intercepted by the marking on the glass, or they could not possibly be seen; and we have then to consider the effect of interference fringes, as well as the quality of the microscope itself. I do not see that there ought to be any serious difficulty in explaining, on Helmholtz's principles, the resolution of Nobert's nineteenth band. With such an illumination as that adopted by Count Castracane, it appears to me very probable that the interference fringes would so far coincide with the true lines as not to prevent a satisfactory definition. At the same time I am anxious to make it fully understood that in my address I endeavoured more to point out the results that would follow from Helmholtz's theory, than to examine whether it is or is not in every respect true. I should be one of the last to wish it to be looked upon as a final solution of the problem. I think many questions remain to be cleared up by the actual observations of persons conversant with the theory, and accustomed to the practical use of high powers. I am also inclined to believe that several crucial tests ought to be examined. Amongst these I would especially suggest the study of fine lines at very close yet *unequal* intervals, and of lines at equal intervals with one or two *missed out* here and there. Theory indicates that such tests would be far more difficult to see correctly than lines ruled at regular and equal intervals; and an examination of such tests ought to afford much information respecting both the final powers of our microscopes and the physical constitution of light itself. Helmholtz, of course, assumes the truth of the undulatory theory; and though in the highest degree probable, it would perhaps be premature to conclude that it is absolutely certain, when applied to the explanation of every phenomenon, especially in such a case as that under consideration."

Diatomaceæ Absorbed in their Entire State by the Roots of Plants.—Some very curious observations have been made by Professor P. B. Wilson, of Baltimore, U.S.A., which seem to show that the Diatomaceæ, when applied to the earth in which corn was grown, absolutely passed in their entire condition through the roots, and were found in the stems of the corn. In "Silliman's American Journal" (quoted by the "Monthly Microscopical Journal," August 1876) Professor Wilson says:—"To demonstrate this theory, my friend G. I. Popplein, Esq., of this city, suggested the application of infusorial earth of the Richmond formation—found in large quantities on the western shore of the Chesapeake Bay—to land sown in wheat. I have obtained straw from wheat so grown, and have found, after it has been treated with nitric acid, and the silicious remains placed on the field of the microscope, that it consisted wholly of the silicious shields of Diatomaceæ, the same as found in the infusorial earth, excepting that the larger discs in their perfect form were absent (*Actinocyclus Ehrenbergii* and *Actinoptychus undulatus*). My conclusions are that they—and there probably may be other forms—are too large to enter the root capillaries. During the coming summer I will attempt, if possible, to make micrometer measurements of both. The discovery of Diatomaceæ in their original form in this wheat-straw precludes the possibility of the infusorial earth having undergone any chemical change in the soil, either by forming chemical combination with the alkalies or the earths, or by suffering physical disintegration from any catalytic action of any salts present in the soil. In the particles of silica placed upon the glass slide, when they were completely separated from each other, the outlines of the individual diatoms were sharply and distinctly defined. On the other hand, when the physical action of ebullition with nitric acid was not sufficient for the complete separation of the particles of the epidermal shield, there was observed a marvellous interlacing of the various forms, showing that they were conveyed by the sap-cells directly to the section of the plant where they were destined to complete its structure. I have examined several specimens of straw, taken at random in the market; the silica in each specimen consisted of plates, very thin, and truncated at the corners."

Musical Sand Examined beneath the Microscope.—A paper on this subject, which is really a somewhat curious one, is published in the last number of the "Proceedings of the California Academy of Sciences" (vol. v). He states that "in order to ascertain, if possible, the cause of the sound that is produced by the sand from Kauai, presented to the Academy at a former meeting, I investigated its structure under the microscope, and I think the facts I have ascertained fully explain the manner in which the sound is produced. As the grains of sand, although small, are quite opaque, it was necessary to prepare them so that they should be sufficiently transparent to render their structure visible. This was effected by fastening them to a glass slide and grinding them down until one flat surface was obtained. This surface was then attached to another slide, and the original slide being removed, the sand was again ground down until sufficiently transparent. The grains were found to be chiefly composed of small portions of coral and apparently calcareous sponges, and presented under the microscope a most interesting object. They were all more or less perforated with small holes, in some instances forming tubes, but mostly terminating in blind cavities, which were frequently enlarged in the interior of the grains, communicating with the surface by a small opening. A few Foraminiferæ were also met with, and two or three specimens of what appeared to be a minute bivalve shell. Besides these elements, evidently derived from living beings, the sand contained small black particles, which the microscope showed to be formed principally of crystals of augite, nepheline, and magnetic oxide of iron, imbedded in a glassy matrix. These were undoubtedly volcanic sands. The structure of these grains fully, I think, explains the reason why sound is emitted when they are set in motion. The friction against each other causes vibrations in their substance, and consequently in the sides of the cavities they contain; and these vibrations being communicated to the air in the cavities, under the most favourable conditions for producing sound, the result is the loud noise which is caused when any large mass of sand is set in motion. We have, in fact, millions upon millions of resonant cavities, each giving out sound which may well swell up to resemble a peal of thunder, with which it has been compared; and the comparison—I know from others who have heard it—is not exaggerated. The effect of rain in preventing the sound is owing to the cavities in the sand becoming filled with water, and thus rendered incapable of originating vibrations."

PHYSICS.

A New Form of Mountain Barometer has been described by Mr. Emmons in "Appalachia," June 1876, the first number of a new American journal devoted to mountaineering pursuits. In the ordinary mountain barometer we must carry about with us a tube of nearly a yard in length, but Macneill's instrument need not be more than half this length. The tube is open at both ends, and the lower end passes by an air-tight connection through the top of a cylinder, which opens only in a tube below, connecting it with the mercury cistern. The bottom of this cistern is of soft leather, resting upon the end of a vertical screw, as in the Fortin barometer, and the whole cistern, with its mercury, may be separated from the tube and carried independently, the liquid being retained by a stop-cock. To use the instrument, it is hung vertically, and the screw is turned up, forcing the mercury into the cylinder above. Then the liquid rises to the level of the bottom of the open tube, the air above it is confined in the cylinder, and is then under the atmospheric pressure. Now, if we continue to force up the mercury we compress the air in the cylinder, and the liquid rises to a corresponding height in the tube. This compression is continued in all measurements, until the mercury in the cistern rises to a certain fixed point. The corresponding height of the mercury column in the tube, as read upon its scale, will be greater or less, according as we are reading at the sea level or upon a mountain, and if the scale has been previously graduated by comparison with an ordinary mercurial barometer at different pressures, the atmospheric pressure may be read directly from it. The error in reading this instrument is greater than in the Fortin or other barometers, and it is also liable to error from some other sources; but it is an extremely convenient form for transportation, since the mercury may be carried separately from the tube, and the whole instrument is hardly more than half the length of the shortest syphon barometer of the same range.

New Experiments with the Radiometer have been conducted by M. A. Ledieu, and are reported as follows in the "Comptes Rendus" (June 12) and "Chemical News" of July 14:—The radiometer was found to continue revolving when submitted exclusively to a pencil of luminous rays falling parallel to its axis. The author, however, does not draw the conclusion to which a superficial and systematic examination of this result might seem to lead. The experiment performed by M. Salleron at the suggestion of the author, condemns decidedly the doctrine of emission as an explanation of the movement of the radiometer.

Experiments with Frozen Dynamite.—Some interesting experiments were recently made at the works of the British Dynamite Company, at Stevenston, Ayrshire, with the view of proving that dynamite in a frozen state is as safe to handle and to transport as in an unfrozen state. They are reported in the "Chemical News," July 14. Professors James Thomson and Bottomley, of the University of Glasgow, were present. In the first experiment, several cartridges in a frozen state, and in some parts beginning to thaw, were thrown one by one from the hand, with great force, against an iron plate, without explosion. In the second experiment, a block of iron, about 400 lbs. weight, was allowed to fall from a height of about 20 feet on a light wooden box containing 20 lbs. of dynamite cartridges in a frozen state, and with slight signs of incipient thawing in spots more exposed to the warmth of the air. The box was smashed, and the cartridges were crushed flat and pounded together, but there was no explosion. The crushed cartridges were next made up into two heaps to be exploded. The ordinary detonator shatters, but does not explode the frozen dynamite. The explosion was therefore effected by inserting in each heap a small unfrozen cartridge, with the ordinary detonator inserted into it, and then firing this off by a Beckford fuse. The two heaps were exploded successively, and it is worthy of remark that the explosion of the first, though very violent, did not set the other off.

Sound Attractions.—A recent number of "Silliman's American Journal"—which, by the way, is remarkable for its admirable summary of Physical Science—gives the following account of M. Doorak's recent inquiries. It says:—"M. Doorak has examined the attractions and repulsions of small pendulums hung near sonorous bodies. A square of paper or a piece of cork is hung by a silken thread, and held near a wooden rod, vibrating slowly. Varying the positions of the pendulum, it is sometimes attracted and sometimes repelled. These motions seem to be due to the air-currents approaching or receding from the rod, and the motions of the cork served to determine approximately the directions. These results were verified by the motions of a flame and the indications of a very sensitive water manometer. The air thrust aside by the vibrating rod escapes laterally, repelling light bodies. This is replaced by air forming counter-currents toward the rod, producing the effect of attraction. When the amplitude of the vibrations is small, the rod acts like the prongs of a tuning-fork, and attraction takes place in every direction. In front of the opening of a tube of Kundt, is placed a second open tube, giving the same sound as the first, and suspended by two threads. Making the first tube resound loudly, the second tube is strongly repelled. The same effect is obtained if the second tube gives one of the harmonies of the first. Placing two tubes facing each other opposite the tube of Kundt and perpendicular to its axis, they tend to approach each other. With a very sensitive manometer it appears that in a column of air in a state of permanent vibration, the air at the nodes has an excess of pressure. This accounts for the heaping up of water in the loops of a tube of Kundt. It is explained by admitting that the amplitude of the vibrations cannot be neglected compared with their length. It follows that there ought to be a continuous motion of the air from a node to a loop. This might be proved by filling the resonant box of a tuning fork with the fumes of chloride of ammonium, and seeing if they are thrown out when the fork is set in vibration. If a bell is filled with water, and a drop of oil allowed to fall on it, the circular film becomes quadrangular when the bell is sounded. The water-currents start from the nodes, and accumulate at the loops. A disk of glass is attached to the end of a rod vibrating longitudinally. If a glass drop is hung opposite the disk, it will be repelled at the centre and attracted around the periphery. There are then, as with air, currents outward at the centre, and counter-currents inward along the edges.

Temperature of the Interior of the Earth.—From observations made on the well of Sperenburg, near Berlin, M. Mohr ("Les Mondes," May 4) concludes that at the depth of 5,170 feet the increment of heat must be nil. A similar decrease of the increment of heat has been observed in the Artesian well of Grenelle. Hence M. Mohr draws conclusions unfavourable to the Plutonian theory.

ZOOLOGY AND COMPARATIVE ANATOMY.

Impregnation of the Boa-Constrictor.—In a recent number of the "American Naturalist," Mr. S. Lockwood makes some interesting observations on the eggs of the above animal—in fact, he puts a very important question to the physiologist. He says: "My friend Dr. Kunzé has shown me an infertile egg of a boa which he lately obtained at the Central Park menagerie. The boa laid twenty-one eggs, each about the size of a hen's egg. The animal made the deposit in sight of her keeper and others. She laid two fertile eggs, and then a sterile one, in regular succession; each third egg was sterile. The fertile eggs had each a young boa within. One came out of its shell immediately after being laid, but soon died. All the others died within their shells. The sterile eggs were albuminous throughout, and cut like cheese and smelled like sperm-oil. Could this be the balance of an impregnation received the year before?

The Cat as a Substitute for the Carrier Pigeon.—It seems that the Belgians have formed a society for the mental and moral improvement of cats. Their first effort has been to train the cat to do the work now done by carrier pigeons. The most astute and accomplished scientific person would have his ideas of locality totally confused by being tied up in a meal-bag, carried twenty miles from home, and let out in a strange neighbourhood in the middle of the night. This experiment has, however, been repeatedly tried upon cats of only average abilities, and the invariable result has been that the deported animal has re-appeared at his native kitchen-door the next morning, and calmly ignored the whole affair. This wonderful skill in travelling through unfamiliar regions, without a guide-book or a compass, has suggested the possibility of cats being used as special messengers. Recently thirty-seven cats residing in the city of Liege were taken in bags a long distance into the country. The animals were liberated at two o'clock in the afternoon. At 6·48 the same afternoon one of them reached his home-

His feline companions arrived at Liege somewhat later, but it is understood that within twenty-four hours every one had reached his home. It is proposed to establish, at an early day, a regular system of cat communication between Liege and the neighbouring villages.

A Cosmopolitan Butterfly.—Mr. S. Scudder gives the following sketch of the distribution of *Vanessa cardui* in the "American Naturalist" for July:—"There is but one butterfly whose range is so extended as to merit the name of cosmopolitan; it is the Painted Lady, or *Vanessa cardui.* With the exception of the Arctic regions and South America, it is distributed over the entire extent of every continent. Australia and New Zealand produce a race peculiar to themselves, while the other large islands south of Asia possess the normal type, which is also found upon small islands lying off the western borders of the Old World, the Azores, Canaries, Madeira, and St. Helena. On the other hand, it has not been discovered upon the small islands off the American coast, such as Guadaloupe, the Revillagegidos, and Galapagos on the western side, or the Bahamas and Bermudas on the eastern; neither does it occur in any of the Antilles, excepting Cuba, and there but rarely. It is reported, however, from islands lying in the middle of the Pacific Ocean, such as the Hawaiian group and Tahiti, but its actual occurrence there is at least doubtful. On the American continent, its southern boundaries will probably be found in Venezuela, New Granada, and Ecuador, but it is abundant even as far south as the highlands of Guatemala, and thence stretches northward over the entire breadth of the continent to the Arctic regions; on the eastern coast it has been found as far as Labrador, and on the west to the eastern shores of Behring's Straits. In the heart of the continent I have taken it upon the Saskatchewan, and Doubleday reports it from Martin's Falls; but Mr. W. H. Edwards does not recollect seeing it in the few collections he has examined from points farther north. As we see it flourishing in the colder regions of Europe and North America, so also is it found on all mountain heights; and Mr. H. W. Bates, writing of the whole genus, distinctly says it is 'found only in elevated places in the neighbourhood of the Equator.' The stations in Southern Asia from which *V. cardui* has been reported—Cashmere, Nepaul, Bootan, and Sikkim—all lie on the flanks of the Himalayas, and the Nilgherry Hills are the highest elevations of the Indian peninsula. In the Alps of Europe this insect flies to the snow level; but in North America, although it may be regarded as one of the commonest butterflies in the elevated central district, it is most abundant at a level of 7,000 or 8,000 feet. Lieut. W. L. Carpenter and others have never found it above the timber line; but Dr. A. S. Packard, jun., has taken it on Arapahoe Peak between 11,000 and 12,000 feet, and on Pike's Peak from 8,000 to within 500 or 1,000 feet from the summit."

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